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Psychological Review

EDITED BY

HERBERT S. LANGFELD, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
W. S. HUNTER, CLARK UNIVERSITY (*Psychol. Index*)
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Joseph M. Peterson

1878–1935

VOL. 43, NO. 1 JANUARY, 1936

THE PSYCHOLOGICAL REVIEW

JOSEPH PETERSON

EDITOR, PSYCHOLOGICAL MONOGRAPHS, 1934–1935

A hundred years hence the historian may be able to write that psychology grew into a mature science during the first half of the twentieth century. He will possibly observe that by 1950 most psychologists had come to substantial agreement with respect to fundamental postulates, descriptive categories, fruitful problems, and useful methods of investigation. If this prophecy be justified, and recent trends in psychology lead one to hope that it is, then the historian will find few men whose work reflects more adequately the spirit and promise of the period than that of Joseph Peterson. His impatience with futile controversies among the 'schools,' his predilection for crucial experiment rather than arm-chair systematization, his persistent attack upon faculties and mechanical association principles, his organismic conception of adaptive process—these attitudes throw into sharp relief both the man and his time. The career of such a man is, therefore, of great interest to psychologists, as much for its symbolic significance as for its intrinsic contribution.

The details of educational background and academic affiliation are ordinarily of minor importance in an appraisal of the work and influence of a man of science. In the case of Peterson, however, certain items of this nature serve better to reveal his character than almost any other form of description. He was twenty-six years old and had been married a year when he came to the University of Chicago in 1904 to complete baccalaureate work begun in the colleges of his native state, Utah. He had taught in the public schools of

Utah and Idaho since 1899, and his college attendance had been restricted mainly to summer sessions. In spite of almost insuperable financial odds, he managed to remain at the University of Chicago continuously until 1907, earning in that period both the baccalaureate and the doctorate degrees. He returned to Utah in 1907 to teach in Brigham Young University, the school of the Mormon church. But his firm scientific convictions brought him into such conflict with denominational tenets that he resigned in 1911, along with other 'liberals' in the faculty. From 1911 to 1915 he taught in the University of Utah, resigning this position in protest against the administration's treatment of another instructor. It was then that a chance malicious remark by a member of the administration faction provoked a decision which influenced Peterson's entire career, perhaps adversely from many points of view. This individual observed that Peterson could afford to resign in 'indignation,' since he was fairly certain that the University of Chicago would offer him a position. It so happened that he did receive an invitation to return to his alma mater, but he declined it, both because he did not wish to have the motive for his resignation impugned, and because he felt that the offer might have been made mainly out of consideration for his circumstances. So he accepted a professorial lectureship at the University of Minnesota, receiving a permanent appointment as assistant professor the following year. In the spring of 1918 he was made chairman of the Department of Psychology at Minnesota, but resigned that fall to accept a professorship at George Peabody College for Teachers, in Nashville. The climate, the belief that Nashville would grow into a great university center, and prospects for an extensive development of his own laboratory were important considerations which influenced him to come to Peabody College. He remained there until his death, declining offers which came to him from time to time from other institutions. Of these, none pleased him more than the invitations to return to the Utah colleges which earlier had found his ideas so uncongenial.

These fragments from Peterson's academic career suggest

something of the intellectual honesty and moral scrupulousness which were his outstanding personal characteristics. He always professed the most complete contempt for those fellow psychologists whom he suspected of subversive self-promotion practices. He was unpopular with many of his colleagues because of somewhat caustic reviews and comment upon work in which he detected faulty methods or inferences. Yet, paradoxically, his uncompromising personal and professional integrity was probably largely responsible for the high esteem in which he was held by most of his fellow scientists, a feeling evidenced by the many honors which he received at their hands. Peterson held almost every important office in the psychological associations with which he was affiliated. He was a member of the Council of Directors and later President, both of the American Psychological Association and of the Southern Society for Philosophy and Psychology. His address as retiring vice-president of Section I of the American Association for the Advancement of Science was to have been delivered at the Washington meeting held in December, 1936. He served for many years on committees of the National Research Council and of the Social Science Research Council, and at the time of his death he was editor of the Psychological Monographs, book-review editor of the American Journal of Psychology, and associate editor of Mental Measurement Monographs.

Considering the duties incident upon such activities, and his excessive teaching schedule, it is amazing that Peterson could find any time for research and writing. He taught eleven months every year for almost twenty years, conducting three or four classes almost without assistance. He practically never had a vacation, since he usually spent the time between summer and fall sesssions in a furious effort to complete pressing tasks. Yet a complete bibliography of his writings contains more than a hundred titles, exclusive of book reviews. Many of these publications were minor articles, notes, and abstracts of papers read at meetings, but his major contributions in audition, learning, intelligence and its measurement, and race differences constitute an imposing monu-

ment to a man who literally gave his life to psychology. Such diversity of research interests is probably uneconomical from the point of view of individual accomplishment, but psychology may have profited as much from Peterson's versatility as if he had worked exclusively in one field. His interest in a problem guaranteed a discriminating analysis of its fundamental elements, an exposure of any illicit procedure that might characterize other work on the problem, and, usually, fruitful hypothesis and experiment.

Boring has written that American psychology is typically a psychology of capacity, or of individuals, and that in the field of general experimental psychology there is no distinctively American contribution. I should like to take exception to this generalization with particular reference to the problem of the learning process which is mainly American in origin. Peterson considered the learning process to be the most fundamental problem in psychology, and to this field—in which he included perception—he contributed some of the most ingenious conceptions and experiments recorded in the literature. His first paper on learning appeared in 1916, and it included both a statement of his "completeness of response" hypothesis and a criticism of traditional association theories. This paper and the subsequent experimental articles on learning constitute about as meaningful an organismic theory of behavior as I have encountered. It should be noted in this connection that Peterson objected to the use of the phrase 'organism as a whole' as a question-begging epithet, designed to solve in a general verbal gesture all of the psychological problems with which one might be having conceptual difficulty. He simply saw that the observable phenomena of learning could not be explained in terms of serial associations among discrete acts, and that the assumption of some mysterious effect of 'pleasure' in reinforcing such 'bonds' did not materially clarify the problem. It was his view that by means of complex neural mechanisms of interstimulation the residual effects of incomplete reactions—those reactions failing to resolve the internal tensions generated by the motivating conditions—would persist in the form of a

cumulative neural disposition and would finally be associated with the complete or consummatory response "by what is tantamount to simultaneous action." It seemed to him to be necessary to assume an overlapping of stimulus effects and a final physiological resolution of all relevant responses into a consummatory reaction in order to account for the larger orientations of animals to total situations. Peterson then began to devise experiments to check the validity of his own and other hypotheses concerning the nature of the learning process. Working with white rats, he found that the longer blind alleys in a maze were harder to eliminate than the shorter ones, which fact presumably indicated that in the case of the former the loss of tentative reactions and attitudes was greater than in the latter alleys. The development of orientation to the whole situation was apparently indicated both by the rat's tendency to maintain a forward direction, and by the occasional wide-spread disorganization which would result from a single error. With respect to frequency and recency effects, Peterson found, upon an analysis of individual reactions in terms of conformity with expectations based upon these association principles, that over fifty percent of the rat's early critical choices in the maze were contrary to these expectations. In later experiments with human subjects in his mental maze, he found that learning occurred even when the experimental conditions were arranged so that the influences of frequency, recency, and 'effect' were definitely unfavorable to the learning of the maze.

Peterson noticed in his study of maze learning that the culs-de-sac near the goal were more easily eliminated than those near the entrance to the maze. This discovery led him to perform a further experiment in which the 'choice' at each bifurcation was determined by tossing a coin, and he found that in this imaginary run also the 'rat' displayed an error-gradient which increased definitely in the goal-entrance direction. Peterson concluded from these probability data that a simpler situation existed at the goal than at the entrance to the maze, and that the organism could, therefore, more easily effect a consummatory response at the goal end.

Or to state the matter more directly in terms of his theory, the incomplete reactions to blind alleys near the goal could more readily be resolved into the consummatory act. These interesting phenomena on the order of elimination of errors in a maze suggested to Peterson a most ingenious neurological hypothesis. Comparing the processes within the nervous system to the behavior of an animal in a maze, he suggested that, on the basis of the same mathematical principles observed in his probability experiment, one might expect to find that the random nerve impulses set up by a motivating situation would be eliminated in the order of their proximity, in neural relationship, to the operation of the mechanism controlling the final consummatory act. Such an hypothesis anticipates, in its general character, certain recent conceptions which hold that psychological patterns may be directly symbolic of the character of nervous operations, and that a psychological approach to neurological problems may perhaps be more fruitful than the converse procedure. Unfortunately, Peterson was never able to put his theory to experimental test, although he referred to it in his last formal paper as a notion which "will not stay down" and which conceivably could be tested experimentally.

Peterson's interest in active investigation never waned in spite of obstacles which would have destroyed it in a less determined man. Just at the time of his death he was preparing enthusiastically to continue a study of perception, which he had begun by repeating the experiments of Stratton and Ewert with lenses which invert the retinal images. He refused to relax pleasantly into the Elysium of abstract theory, which is perhaps the natural prerogative of psychologists past fifty. In fact, Peterson professed an aversion to abstract formal systematization, and never published a paper designed to set forth his 'general' point of view.

Peterson's teachers were Angell and Watson, a fact which might suggest to the mechanical classifier that he would be a combination of functionalist and behaviorist. But labels do not fit Peterson very well. He could speak of his introspection and yet deny the causal efficiency of consciousness.

It should not be inferred from this that he was confused in his thinking. He simply hoped that psychology would become a unified scientific discipline, like other sciences, and he believed that his desideratum could be achieved best by relating theory to concrete experimental problems. His article in the Washburn Commemorative Volume is perhaps his nearest approach to a general theoretical paper, and yet this deals specifically with constructive learning and thinking, with continuous references to experimental findings. His monistic point of view is, however, clearly expressed in this paper. He conceived all adjustive processes to be organic operations rather than arbitrary expressions of extra-organic entities. Ideas, resolutions, will and the like are directive tendencies which arise in and develop out of life processes. The vitalists and dualists generally err, he states, in failing to recognize that 'vital forces' are based on organizations of natural forces which have a natural continuity. Consciousness was considered by him to be simply an 'inner aspect' of organic process; it possessed no causal efficacy and required no external agent for its production. The fact of awareness was characteristic of certain levels of organic activity, but since it was a relational phenomenon of quite instable character, it could not be exhibited in any manner which satisfied the demands of scientific logic.

If it appears from these statements that Peterson was a cold-blooded, behavioristic materialist—a thought which may already have occurred to the philosophically unsophisticated reader—then it should be further recorded that, following James, he believed in the reality of man's creative role in cosmic process. Every individual was held to be a unique organization which reorganizes and gives direction to the diverse processes affecting it. Furthermore, there seemed to him to be no way by which man could play his proper role in the creative process quite as well as by utilizing scientific method. By this means man could secure the knowledge essential to a free, rich existence. Peterson was impatient with those philosophers who would restrict science to the role of supplying the practical needs arising in man's routine

existence. He was even more insistent that philosophic knowledge was not a different, 'higher' variety than scientific truth. Furthermore, he believed that science was a more valid and important procedure than most religions, since all too often religion depended upon a confusion of man's understanding. At the same time he was profoundly religious in terms of sensitiveness to the broader implications of nature, and of hopefulness regarding the ultimate destiny of man in the universe.

LYLE H. LANIER

Vanderbilt University

PSYCHOLOGY AND LIFE[1]

BY ALBERT T. POFFENBERGER

Columbia University

You have assembled for the forty-first time to listen to an address by your chosen representative. Tradition has fairly definitely fixed the nature of such an address in the form of a scholarly presentation of a subject dear to the heart of the speaker. It might, however, take other forms. It might become the medium for a review of the year just passed or the decade just passed, somewhat like the report of a corporation president to his organization. It is customary in such reports to point out successes and failures of policy, and to indicate trends for future development. In our own Association, which has grown so rapidly and changed so materially during the last ten or fifteen years, there are numerous questions of policy that might well be reviewed. Are the classes of membership and the requirements for admission to them just what they should be? Can the service which we render to our membership be improved through reorganization of our publications or otherwise? The Association is now responsible to its members for an imposing array of journals, bibliographies and abstracting services. Should there be elimination, consolidation and improvement among these? Can our whole business and financial structure be modified in the direction of increased simplicity and efficiency? Shall we retain indefinitely, for instance, the American Psychological Association, Incorporated (in the District of Columbia) and the Psychological Review Publications, Incorporated (in New Jersey) as separate institutions with separate financial officers, or shall we consolidate them? Have we outgrown our long established system of annual meetings, and should we substitute for them a regional organization of some sort?

[1] Presidential address delivered before the American Psychological Association, Ann Arbor, Michigan, September 6, 1935.

What can we learn from the practices of related scientific bodies in these respects? Rapidly increasing size has forced the Association to discard more than one of its cherished customs, with the result that impersonal techniques have replaced the more intimate personal contacts within the membership. Should anything be done to bring back the 'good old days'? Are we taking an appropriate attitude toward the practical applications of our science, or are we as an Association holding too much aloof from everyday problems from fear of contamination by them? These questions of policy and many others like them, discussed among individuals and groups, might well be brought into the open through the medium of your president's address.

This annual event might take still another form, namely, an accounting to the public for the activities of the psychologist, or even more pertinently, an offer of our Associations's services as an aid in the solution of governmental and public questions generally. There would be precedent for either of these proposals. The address of the distinguished president of the International Congress of Psychologists at Yale University in 1929 was in the nature of the first and the response of the American Psychological Association to the emergency of the Great War was an instance of the second sort. It may well be that the emergency created by the present depression constitutes just as pressing a call as that of the war period. When could the human problems ever be greater than in a period of social reconstruction such as we are now experiencing? Widespread unemployment calling for adjustment of millions to new tasks, increased leisure for these millions beyond the dreams of a single generation ago, plans and programs for unemployment insurance and old-age insurance, changing educational methods to prepare the oncoming generations to meet the changed social environment—every one of these has psychological problems at its core.

The very depression that initiated these changes has been called a psychological phenomenon. How better could starvation in the midst of plenty be characterized? Let General Hugh S. Johnson be the spokesman for the New Deal on this

matter: "Every element for prosperity is here—money, vast pools of credit, great resources, starved demand, fine facilities for manufacture, sale and distribution, and a brave, active, alert and patriotic people. . . . The single missing element is confidence."

Concerning the value of money he says: "It is a question of what people fear and of how they will act in a panic of fear—as a man will run from a burning house carrying a pillow and leaving his watch and pocketbook to be burned in the place where the pillow rested. In other words, the value of money is a matter of mental process—deliberate and based on facts most of the time, but confused and even lunatic when enough has happened to cause panic fear."

President Roosevelt in his inaugural address remarked that the only thing that this country needed to fear was fear. Confidence and fear, the positive and the negative, are psychological concepts, both of them.

Dr. John C. Merriam,[2] in a recent address on 'Some Responsibilities of Science with Relation to Government,' remarked that it is important that information in the field of science be examined with reference to its possible value in study of major national or governmental projects. The context shows that he did not have the science of psychology particularly in mind when he made that statement but it is equally appropriate there. He says further that "The scientist should not necessarily expect to administer the results of his own work, and yet the relation to administration is extremely important. He cannot avoid considering the broader implications of his contribution . . . if the results of science are to be fitted into the economic or governmental plan."

One of the most frequently cited explanations of the world's present plight is to the effect that the social sciences have lagged too far behind the physical sciences in their practical applications. The trouble is not, the physical scientist insists, that he has introduced too many revolutionary innovations but rather that the social scientist has not

[2] *Science*, 1934, **80**, 601.

shown the way for an adequate and satisfying adjustment to these innovations. Whether social science, biological science, or both, psychology will bear at least a part of the responsibility implied in this accusation. Even within the membership of our own Association, the view is frequently expressed that we should be contributing more actively to the solution of the great social and economic problems that Society faces today. Proposals of various kinds have been made and still others are forthcoming which look toward the stimulation of thought and research in these directions. Witness the recently formed 'Psychologist's League,' organized for two interdependent purposes, one of which is "to stimulate the science and the profession of psychology toward developing an orientation which will be critically aware of and constructively responsive to social realities." Should the American Psychological Association not initiate or at least extend a guiding hand in such a project?

With all the superficial recognition of things psychological in recent events, there was no psychologist in the 'brain trust'; there was no psychological component in the long list of proposals for the contribution of science to a long term recovery program; there was no psychologist included in the Science Advisory Board of the National Academy of Sciences and the National Research Council to advise the government where science touches governmental operations. There is no psychological service set up as an integral part of the Civilian Conservation Corps, that organization whose function it is to salvage, develop and guide the young men of our country. There is no call upon the psychologist for help in the aviation activities of the government in its divisions of army, navy and commerce, where psychological problems of the long recognized 'personal equation' are so numerous. The Wickersham Commission for the survey of crime in the United States included no psychologist. One notable exception to this neglect of what the psychologist has to offer is to be found in the program of the United States Employment Office of the Department of Labor. This exceptional instance is due to the courage and the far-sightedness of a few of the leaders in

this movement, who recognize the fundamentals underlying the problems of employment. But even here the success of the psychological contribution seems to depend upon how well it is kept under cover; however valuable the psychological work may be, diplomacy dictates that it shall function under an assumed name.

How is the state of affairs illustrated by these few random samples to be accounted for?

Does the American Psychological Association, which is our only truly national medium for expression, hide its light under a bushel? Does it draw too sharp a distinction between the professorial and the professional? Does it without protest permit psychology to be sold to the public by the amateur, the mountebank, or still worse, the charlatan and the purveyor of pornography through the medium of the newspaper, the cheap magazine and the radio, while its own pronouncements are carefully guarded within the covers of its scientific journals? Is anything being done to give our well trained but unemployed young psychologists an even chance to compete with those who are both untrained and unscrupulous?

Your president realizes that he has been delegated neither to dictate matters of policy nor to offer your services to the public. But the foregoing random observations raise two fundamental and related questions which he ventures to discuss. These are: "What is the 'homo psychologicus'?" and "What is his relation to life?" The speaker is not capable of giving a final answer to these questions, but he hopes to show at least the need for answers and to point out the form that the answers might profitably take.

To obtain a description of *homo psychologicus* either of two techniques could be employed. We could see ourselves as others see us, by calling perhaps upon the much overworked man from Mars, or we could institute a self-examination. The former would be healthful, but might be rather too painful for an occasion of this sort. To such an extra mundane observer the species psychologist would include the following: the editor and contributors of 'Sex Psychology,' a magazine which answers 'modern sex problems,' appearing

first in March, 1935, and which contains among other articles ones on 'Petting and Love' and 'Phallic Cults in Harlem.' It would include the radio crooner who promises to soothe the troubled mind, whatever the nature of its troubles. It would include the fortune teller, the palmist, the astrologer and the interpreter of tea-cup dregs. It would include the psychoanalyst, the vocational counsellor, the popular lecturer. And lastly, it would include the teacher of psychology and the investigator in the college and university. Such is the kaleidoscopic and confusing picture that our profession presents to the layman, and, I fear, to many of our sister sciences.

Seen from within, the picture is less embarrassing although it is not much less confusing. Preparation for the career of psychologist would vary all the way from sitting as a child upon William James' knee to the most rigid scientific apprenticeship. The activities in which he is seen to be engaged would be those of mathematician, physiologist, physicist, educator, dean, university president, advertising agency official, and housewife, to mention only a few.

But even if we confine our inspection to the ranks of our Association, great divergences appear. The findings of the recently appointed committee on the Ph.D. in psychology have a pertinent bearing upon this matter. The preparation for advanced study required by the different degree-conferring institutions covers the widest range, from a strict training in the physical and biological sciences, to no such requirement. In fact, there would appear to be at least two fairly distinct sub-species of homo psychologicus: the one developing from a background of philosophy, education and sociology; the other from a background of physics, chemistry, biology and physiology. The two may scarcely speak the same language. The one common ground seems to be training in statistics, although competence in the use of this tool varies between the groups. It might be inferred that the former group would become the sub-species Social Psychologist, and the latter the sub-species Biological, Physiological or Experimental Psychologist. If this were so, one might justify the distinction in preliminary

training on the ground that the problems to be met by the two sub-species call for a different perspective. However, the strains of these two sub-species become intermingled with the result that psychologists differ radically among themselves in background and outlook. Add to such variations those which arise from differences in residence requirements, in comprehensive examinations, in competence and point of view of teaching staffs, in library facilities, in the opportunities for field training; and the expected diversity will just about match the actual diversity.

Such variations within the species take on greater and greater importance as the contacts between the psychologist and the world at large occur on an ever widening front. The label 'Psychologist' must eventually come to stand for something in the way of uniformity and stability of the product, both for the good of the public and for the psychologist himself. Such stability has been attained in the realms of medicine and engineering, although to maintain it involves a continual vigilance. The engineer need no longer be confused with the engineman, nor the medical doctor with the chiropractor. The psychologist should expect no easier task. It will be pointed out, no doubt, that medicine and engineering are applied sciences and that therefore psychology as represented in this Association cannot properly be compared with them. But psychology is almost inevitably an applied science, as I hope to show later, at least in so far as its contact with practical human problems is concerned.

Much can be learned from the history of medicine that can be directly applied to the problem which the psychologist faces. A superficial study of this evidence might lead to the conclusion that the medical profession attained its present position of dignity through fighting imposters and charlatans. This, I venture to suggest, has been only a very minor part of the process. One of the aims of the American College of Physicians is thus stated: "to *hallmark* those who have equipped themselves by special study and application to practice one or other of the senior branches of the medical art and science as distinct from those who have not so prepared them-

selves, but would have the public believe that they have, and so profess to all and sundry."

The principal function here is the hallmarking of the fit rather than the fighting of the unfit. And before the fit can be hallmarked, the condition of fitness must be established. This is the major task. It has taken years of effort to establish throughout the country the minimum specifications for fitness of the doctor of medicine, and to have these specifications recognized both by the law and by the layman. The psychologist faces the same need. Before he can drive the imposter from the field, he must first determine without the shadow of a doubt who are imposters and who are not. He must define *homo psychologicus* in terms of minimum training and experience, and set up the machinery for stamping the product.

The attempted certification of clinical psychologists by our Association beginning in 1917 was an error and was rightly abandoned, but not because such a function was necessarily outside the sphere of the organization. It was rather because there was lack of unanimous agreement as to just what the specifications for a clinical psychologist should be. And back of that there were no generally accepted specifications for psychologists. The latter must precede the former. Again we refer to the profession of medicine. In spite of its great progress, there are still no specifications for medical specialities such as surgery and psychiatry. Enough, then, to look forward for the present to the standardization of the psychologist as a species by setting up at least a minimum of requirements.

There might be recalled, too, the attempt in 1917 to control publications in Applied Psychology through the medium of a permanent committee of the Association and for the purpose of keeping 'the work in applied psychology free from charlatanism.' This effort was the expression of a real need even at that early date, but the time was not ripe and the method of procedure not clear.

Who shall take the responsibility for this important definition of terms, and this setting up of standards? The

view seems to be current that this function does not rightly devolve upon the American Psychological Association as a scientific body, but belongs rather to such an avowedly professional organization as the Association of Consulting Psychologists. It should not be difficult to demonstrate, however, that there is no other group than the American Psychological Association sufficiently disinterested, sufficiently powerful and sufficiently representative of the country as a whole to perform this service. The Association of Consulting Psychologists is now a local organization,[3] with no funds, and what is more important still, dependent for its success upon the existence of such standards. Once standards have been established, it will be the privilege and the duty of the Association of Consulting Psychologists, or similar professional organizations, to abide by them and to enforce them. But such interested groups should not be called upon to originate them. There are, doubtless, in the ranks of the Association of Consulting Psychologists those who are not properly qualified to be there. Some of these persons, if not all, fail to fulfill the requirements as psychologists. Their shortcoming lies in their fundamental psychological training. Upon this it is difficult for the professional association to pass judgment. It would seem, therefore, to be the legitimate function of the American Psychological Association to define the species *homo psychologicus* and to set up such minimum standards as may be necessary to satisfy the definition. This is the first and essential step in a needed movement toward the protection of both the psychologist and the public.

The concluding paragraph of Fernberger's excellent historical summary of the American Psychological Association [4] contains these significant words: "The Association has completely and signally failed at every direct attempt to control psychology or psychologists, whether it has been in matters of terminology, technique, or personnel. . . . All such attempts at direct action have sooner or later completely

[3] However, it is rapidly becoming national in the geographical distribution of its membership.

[4] S. W. Fernberger, The American psychological association, *Psychol. Bull.*, 1932, 29, 89.

collapsed without direct effect. . . . Perhaps the outstanding thing which the Association could now do for psychologists would be to work for the establishment of some sort of a legal definition of psychologist, of the sort that the American Medical Association has accomplished for physicians in this country."

This definition of *homo psychologicus* will have to be broad enough to include the various activities which our Association accepts as legitimate. It is not necessary here to recite the qualifications for membership and for associateship, but it is worth noting that the 1935 Directory shows that of the total membership some 530 persons have no association with an academic institution. A surprisingly large proportion of this number are engaged in some form of direct service to people, and indeed most of them would be classed as clinical psychologists. With the predicted limitation of growth within our academic institutions during the next decade, this proportion of the non-academic psychologist is certain to increase.

It is unnecessary to point out that somewhere there must rest the responsibility for seeing to it that this third of our membership is properly qualified for its duties. Few of our degree granting institutions provide the necessary training, either because tradition is against such so-called professional training, or because there is a failure to recognize that psychologists in large numbers hold anything but academic positions, or because adjustment to the changing needs has been too slow. A survey on a grand scale is in order, covering the requirements for the most active professional groups, and the ways in which these requirements may be best satisfied. At the risk of serious opposition I hazard the opinion that this function, too, should be performed by the American Psychological Association through the medium of carefully chosen committees provided with all necessary facilities for a difficult and laborious task.

The survey by your Committee on the Ph.D. in Psychology showed too many qualified psychologists in the ranks of the unemployed, and the number increasing at an alarming rate. Radical contraceptive measures have been proposed

and are being employed in some institutions. A counter proposal which deserves equal consideration is that the field for psychological service should be extended. The previously mentioned Psychologist's League has drawn up a series of proposals covering work projects for psychologists which it intends to submit to governmental agencies. It includes, among other activities, a psychological service in the schools, in the courts, in neighborhood clinics, in industrial training, in institutions for the criminal and the maladjusted. This is a commendable effort and shows what might be done if an extensive and painstaking survey were conducted. Why should an organization of the type of the Psychologist's League have to be set up unless these very pressing problems were being neglected by existing agencies? Our association cannot long hold itself aloof from such practical considerations and still maintain its prestige while other more aggressive groups solve our members' problems.

Our second question already touched upon in what has just been said concerns the relation of the legitimate psychologist (or *homo psychologicus*) to life and its problems. The atmosphere of pure psychology is becoming increasingly contaminated with the breath of applications so that to some critics there seems no longer to be a pure psychology, and to others there seems no longer to be an applied psychology distinct from pure psychology because the two have so thoroughly fused. The eminent dean of American psychologists, whom we delight to honor this evening, said in 1903:[5] "The nineteenth century witnessed an extraordinary increase in our knowledge of the material world and in our power to make it subservient to our ends; the twentieth century will probably witness a corresponding increase in our knowledge of human nature and in our power to use it for our welfare." To him both the increase in knowledge and its use for our welfare were and still are the legitimate functions of the psychologist.

More recently, William McDougall,[6] representing an entirely different school of thought, defined psychology thus:

[5] J. McK. Cattell, *Science*, 1903, **17**, 569.

[6] W. McDougall, Prolegomena to psychology, PSYCHOL. REV., 1922, **29**, 1.

"The aim of psychology is to render our knowledge of human nature more exact and more systematic, in order that we may control ourselves more wisely and influence our fellow-men more effectively." Here too we have the practical forming an integral part of the definition of our science.

If further evidence of the essentially applied nature of psychology were needed, it could come from the statistical studies made from time to time of the activities of the membership of the American Psychological Association. Boring,[7] in his survey published in 1920, showed that some eleven percent were teaching applied psychology, while a smaller number were engaged in research upon applied problems. Terman [8] reanalyzed these data, and by a reclassification raised the figure for research to 53 percent. Fernberger [9] in 1928, following the classification of Boring, found about 30 per cent engaged in instruction in Applied Psychology and almost 50 per cent engaged in research in that general field. After making due allowance for the elasticity of the statistics here involved, and accepting the definition of psychology one time proposed as 'that which a psychologist works at,' one can only conclude that psychology is essentially an applied science.

However we may define the term, psychology is inevitably close to the problems of life; the problems of life are in one way or another its problems; and the solutions of these problems in the laboratory stand to become the solutions of the problems of every day life. The psychologist, on this account, bears a particularly heavy responsibility. Since he works with material of practical import, he must see to it that his pronouncements are as free from error of fact or error of interpretation as is humanly possible. Furthermore, he must follow up the applications of his findings so that errors of either sort, when they do occur, may be corrected with as little delay as possible.

Merriam, in the address previously quoted, issued a

[7] E. G. Boring, Statistics of the American psychological association in 1920, *Psychol. Bull.*, 1920, 17, 271–8.

[8] L. M. Terman, The status of applied psychology in the United States, *J. Appl. Psychol.*, 1921, 5, 1–4.

[9] S. W. Fernberger, PSYCHOL. REV., 1928, 35, 447 ff.

warning to science that the psychologist may take particularly to heart. He says, "While it is necessary to remember that he (the scientist) will be held responsible if he fails to set forth the worth of his discoveries, it is also important to realize that science will be held responsible if over-emphasis is given to isolated groups of facts without reference to their real human significance. It is the responsibility of science to state the truth cautiously, and with care that harm be not done. The manner in which a thing is presented sometimes goes far to nullify the value of what may have great intrinsic importance."

A specific case, which is somewhat trivial perhaps, will serve to illustrate the point. The psychologist discovers that children in the lower school grades are being taught to read print and to write script. Now these two systems of characters are rather different, so that the two forms of instruction do not supplement each other as they might in the early stages of the learning process. It was found upon experimenting that children who were taught manuscript writing learned both to read and to write more readily because the characters that they wrote were the same as those they were reading. Hence, manuscript writing became the accepted form. This is one group of facts. Another group of facts not taken into account brings hardship to children thus taught. In life script writing is still the accepted form, and there are cases in which printed signatures are not acceptable. In fact, in the upper grades of one school, where the manuscript system was taught in the lower grades, the children were required to *write* and were reprimanded for their inability to do so. Special instruction in writing had to be instituted in the upper grades, and personality conflicts were numerous. The experimental findings were correct within their limits, but were applied without full reference to their real life significance.

The point will be further illustrated with a few familiar cases showing the importance of the contributions to everyday life, the extent of the application and the dangers arising from the dissemination of error.

Let us begin by going back to the first decade after our own Association was founded. It was then that the concept of Formal Discipline received its death blow and was interred, or shall I say, cremated, at the hands of the psychologist. The psychologist's substitute for the concept of Formal Discipline in the name of Transfer of Training has had the most profound effect upon education everywhere. It has changed the theory, method and subject-matter of the educational process.

In the last decade of the nineteenth century two sets of facts prepared the way for the psychologist. The increasing industrialization of the country focussed attention upon the need to educate the young people to enter industry, to provide them with the tools of their trade, so to speak; whereas the spread of the secondary school emphasized formal education. The public schools were not preparing the young people for their inevitable life work. Pressure was being exerted in the direction of a more practical type of training.

The second set of facts comes from an entirely different source, namely, neurophysiology. It was in the year 1891 that Waldeyer offered to the world the concept of the neurone, chains of neurones and the synaptic connections among them to replace the older concept of a nerve net system. The new concept provided insulated conduction paths and localized interconnections, suggesting the analogy with a telephone or telegraph system. It replaced a unified system over which impulses spread according to their intensity after the fashion of ripples upon a placid pool.

The concept of Formal Discipline fitted into the older neurological view but not into the newer one. Formal Discipline implied a spread of influence within large areas corresponding to the Faculties. Training developed an area corresponding to the *faculty*, and not a limited neurone chain. One quotation will serve to remind you of the point of view then current:

"The study of the Latin Language itself does eminently discipline the faculties and secure to a greater degree than that of the other subjects we have discussed the formation and

growth of those mental qualities which are the best preparatives for the business of life—whether that business is to consist in making fresh mental acquisitions, or in directing the powers thus strengthened and matured to professional or other pursuits." [10]

Upon the background of new neurological theory and dissatisfaction with the practical outcome of education, there appeared the psychological experiments of Woodworth and Thorndike on Transfer of Training. One of the authors writing in 1915 said:

"When the three papers by Woodworth and Thorndike appeared in 1901, describing the limited extent to which practice in sensory discrimination, the observation of small details, and the like, spread beyond the specific abilities trained, they aroused surprise and incredulity." [11]

Another writer reports that the effects of these experiments "were like a bomb shell dropped into the camp of orthodox complacency." [12]

This revolutionary doctrine answered the question: "Does improvement in one mental function affect others?" in the following way: "A change in one function alters any other only in so far as the two functions have as factors identical elements. The change in the second function is in amount that due to the change in the elements common to it and the first. The change is simply the necessary result upon the second function of the alteration of those of its factors which were elements of the first function, and so were altered by its training. To take a concrete example, improvement in addition will alter one's ability in multiplication because addition is absolutely identical with a part of multiplication and because certain other processes—*e.g.*, eye movements and the inhibition of all save arithmetical impulses—are in part common to the two functions. . . . By identical elements are meant

[10] Joseph Payne, Lectures on the science and art of education, New York: Kellogg, 1885, p. 264.

[11] E. L. Thorndike, Educational psychology (briefer course), New York: Teachers College, Columbia University, 1914, p. 273.

[12] B. H. Bode, Conflicting psychologies of learning, Boston: Heath, 1929, p. 47.

mental processes which have the same cell action in the brain as their physical correlate." [13]

Again, in 1915, one of the authors said that such a limited spread of training as just described would at that date be taken almost for granted. Such is the speed with which this new concept seized the imagination. However, to quote again from the same author:

"The experimental results, indeed, have tempted certain writers to proceed too far toward the absurd conclusion that all practice is utterly specific in its effects—confined absolutely to just the particular situations that were met in the special training and to just the particular habits that were formed."

The original studies lent themselves to such misinterpretation by their emphasis upon *limitation* of spread of influence as against unlimited spread, and by their use of the term *identical*. Hundreds of researches and critical studies of transfer of training have been published since 1901, and in these there is evidence that the misinterpretation still persists. Witness the monograph study published by Orata in 1928 on the 'Theory of Identical Elements.'

The crux of the matter has been in the interpretation of the concept of identity of elements. The formulation of the so-called laws of transfer and their expression in terms of symbols favored interpretation by the careless as utter specificity. Thus, the bonds $ABC \rightarrow 1, 2, 3$ and $XYZ \rightarrow 48, 49, 50$ being strengthened, the bond $A, B, C, X, Y, Z \rightarrow 1, 2, 3, 48, 49, 50$ is strengthened. Reference to neurones or nerve cells, and synapses contributed to the same end, also. Once this notion had become fixed, it was difficult or impossible to eradicate it entirely. Innumerable illustrations of the same phenomenon of the fixation of error may be found in other sciences.

The original studies of Woodworth and Thorndike, followed by those of Ruger, Ruediger and a host of others, have actually shown that the so-called elements are not limited to

[13] E. L. Thorndike, Educational psychology (briefer course), New York: Teachers College, Columbia University, 1914, p. 268.

such functions as adding two numbers, but comprise such general functions as identity of attitude, of method, of emotional tone, of mood, of aim, of procedure, of notions, of maxims, of ideals and of responses to abstract cues. Thus Thorndike speaks of transfer from a science laboratory to everyday life:

"The habit acquired in a laboratory course of looking to see how chemicals do behave, instead of guessing at the matter or learning statements about it out of a book, may make a girl's method of cooking, or a boy's methods of manufacturing more scientific because the attitude of distrust of opinion and search for facts may so possess one as to be carried over from the narrower to the wider field. Difficulties in studies may prepare students for the difficulties of the world as a whole by cultivating the attitudes of neglect of discomfort, ideals of accomplishing what one sets out to do, and the feeling of dissatisfaction with failure." [14]

Granting such elements as the above, Rowe [15] goes still further in saying that "mental attitudes or ideals tend by *chance variation* and by *suggestion* to extend their sphere of action." Just how far chance and suggestion carry transfer beyond the limits of identical elements, he does not say.

One more statement taken from Thorndike tempers still further the shock of the original findings concerning identical elements:

"It must be remembered that a very small spread of training may be of very great educational value if it extends over a wide enough field. If a hundred hours of training in being scientific about chemistry produced only one-hundredth as much improvement in being scientific about all sorts of facts, it would yet be a very remunerative educational force. If a gain of fifty per cent in justice toward classmates in school affairs increased the general equitableness of a boy's behavior only one-tenth of one per cent, this disciplinary effect would be still worth more than the specific habits." [16]

[14] E. L. Thorndike, Principles of teaching, New York: A. G. Seiler, 1906, p. 245.

[15] S. H. Rowe, Habit formation and the science of teaching, New York: Longmans Green, 1909, p. 246.

[16] E. L. Thorndike, Educational psychology (briefer course), New York: Teachers College, Columbia University, 1914, p. 282.

This doctrine, when scientifically interpreted, conformed to the then current neurological theory in its denial of the existence of Faculties, it satisfied the demand that education furnish the tools for every day living and it provided for a general mental discipline through the transfer of the less specific elements. Interpreted more narrowly and less critically, it tended to vocationalize education, to eliminate the so-called cultural studies and to furnish an individual with the tools needed only within his one chosen occupation, providing no background for growth, for meeting changing social patterns nor for the enjoyment of his increasing leisure.

So much for the possible misinterpretation of the concept of transfer of training. In so far as the theory in its original form depended for support upon the neurone concept, certain adjustments may have to be made to meet changing neurological concepts. Or perhaps the transfer that does occur can be explained more readily on other grounds than the presence of identical neural elements. This would seem to be the conclusion to be drawn from the experiments of Lashley which lead him to the concept of 'mass action' and 'equipotentiality.' He says:

"The theory of specificity of conduction paths in habit demands that the reaction learned be given only to the excitation of the same receptor cells as were stimulated during training, or at least a significant number of them. . . . These conditions are rarely if ever met in any training experiment. On the contrary, it is clear in most cases that the response is, within wide limits, independent of the particular sensory cells stimulated." [17]

In his experiments with rats on the discrimination of form in which there was transfer from one form to another, the results could be explained only on the ground that the common elements are the *proportions* of the objects. Or again, in "the transfer of the habit of brightness discrimination from one eye to the other, the same sensory cells were certainly not stimulated in the formation and the performance

[17] K. S. Lashley, Handbook of General Experimental Psychology, edited by Carl Murchison, Worcester: Clark University Press, 1935, p. 474.

of the habit. Where is the common conduction path for the two sensory systems established by training? . . . The common elements in transfer are not common neurons but common ratios of excitation in different neuronic systems."

Concerning the problem of transfer, Lashley concludes as follows: "Our data seem to prove that the structural elements are relatively unimportant for integration and that the common elements must be some sort of dynamic patterns, determined by the relations or ratios among the parts of the system, and not by the specific neurons activated. If this be true, we cannot, on the basis of our present knowledge of the nervous system, set any limit to the kinds or amount of transfer possible or to the sort of relations which may be directly recognized." [18]

Gestalt psychology arrives at essentially the same conclusions, although by a different route. With its doctrine of insight and its physiological concepts of electrical fields and gradients, the only limitation to the transfer of training is set by the capacity for insight or the capacity to respond to gradients.

The relatively new concept of the Mental Trait, far removed as it is from the older concept of Faculties, still bears a resemblance to it and holds possibilities for the expectation of Transfer in every day life. By the correlation techniques certain behavior functions are found to resemble each other, and to differ from still others, *i.e.*, there is a tendency for functions to form clusters. Thus we have become more or less accustomed to speak of a language trait, a number trait, a spatial relations trait, a memory trait, and the like; whereas in the days of Formal Discipline one spoke of a Language Faculty, or a Number Faculty, as well as a multitude of others. Now is it not reasonable to suppose that if functions are thus related, modifications of one function within the trait would influence other functions comprising the trait? This, so far as the speaker knows, has not been demonstrated, but an investigation would be in order to determine the

[18] K. S. Lashley, Brain mechanisms and intelligence, Chicago: University of Chicago Press, 1929, p. 173.

transfer effects of training of functions comprised within a trait as compared with those not so related. Current neurophysiological doctrine would lend some support to this expectation.

There are persons high in educational circles, whether they cling to the old form of Formal Discipline or merely oppose the narrower interpretation of transfer of training, who attribute to modern educational methods the lack of great leaders in our present emergency, and the lack of flexibility among the population at large sufficient to enable them to adjust their minds to change. What element of truth there is in this, and what part of the responsibility, if any, will have to be borne by the psychologist for errors of fact and for the misinterpretation by others of his data, it would be futile to attempt to compute.

Nowhere does psychology touch life today more intimately and on a broader front than in the realm of intelligence measurement. This technique has become an accepted part of the routine mechanics of school and work shop. Its earlier use in determining the relation between mental growth and chronological age in the young is now being extended to cover the relation between mental decline and chronological age in the old. The practical significance of this latter service in terms of human efficiency and human satisfaction will equal that of the measurement of the school child. This single contribution of psychology will compare favorably with any single contribution from any other science in the number of persons whom it affects and in the potential and ultimate benefits that may accrue from it. It was the fear of the consequences of error in intelligence measurement that led some years ago to the violent attack upon the concept of the innateness of intelligence and the feasibility of measuring it once for all. Equally intense and for the same reason was the attack upon the concept of intelligence as the product of environment and its modifiability at will. That controversy has almost subsided. The intelligence test has weathered the storm, and whether rightly or wrongly, intelligence is being measured on every hand. If used in error, immeasurable

harm may be done. Constant vigilance is needed to protect the intelligence test from the taint of commercialism and from other serious abuses.

A more recent contribution, and one which promises large returns in human satisfaction, is the extension of our knowledge of the curve of learning into the adult and declining years of life. The findings here give substantial support to the Adult Education movement and demonstrate the feasibility of education for the leisure which our changing social pattern seems to have in store for us. In terms of sheer human values, it is difficult to compute the worth of knowing that the capacity to learn declines at a surprisingly slow pace and that even in advanced years a fair proportion of one's earlier power is maintained. Clear and unequivocal as these results seem to be, their misinterpretation or exaggeration can lead to economic waste, to distress and disillusionment.

A moment remains for a glance into the future. We see another field that lies near to the hand of the psychologist or is even now under partial cultivation. This is the measurement of the personality. Borrowed from popular usage, the term personality has no well-recognized scientific meaning, but it stands for a group of human characteristics which rival intelligence in their importance for successful achievement. Introversion-extroversion, ascendance-submission, inferiority-superiority, honesty, loyalty, speed, perseveration, are a few of the special aspects of this phase of life that have been marked off for measurement. Unlike intelligence, which has something inevitable about it, personality characteristics are generally thought of as modifiable. Hence to the function of measurement there tends to be added the function of modification of the personality to meet specifications. Great as the hazards are in the development and use of a measuring device, they become still greater when adjustment or correction follows upon the measurement. Terman vigorously attacks the current attempts to quantify personality, and particularly deplores the use that is made of psychological devices by the vocational counsellor.[19] The 'adjustment'

[19] L. M. Terman, The measurement of personality, *Science*, 1934, 80, 608.

of the personality of a child to what is considered to be the norm is a leveling process that is fraught with danger. This is a procedure that would not be tolerated in the case of the intellect. The norm of adjustment, by virtue of its mode of determination, is the average or median. Shall the personalities of the coming generation be ironed into uniformity? To quote from Terman, "The student of genius who is familiar with the motivating influences that have their origin in quirks of childhood personality shudders to think what the result would have been if school counsellors had had a chance to 'adjust' the personalities of the budding geniuses of history. One can imagine them, freed of all their peculiarities and complexes, adjusting to the world as it was, and becoming indistinguishable from the human herd."

Terman might appropriately have quoted the following passage from Burbank,[20] who spoke from his lifelong study of plant life: "One of the greatest fallacies of near science and of amateurs in nature's school is the belief that only from the normal can we get our best development and results. As a matter of fact, Nature shows us again and again that it is from abnormalities that some of our most valuable and beautiful plants arise. From that weak, or abnormal plant—that genius plant—may come the very characteristics that we are looking for, and our only problem is to nurse it physically and keep it strong to pass on its overload of spiritual or esthetic essences to its children."

The power to measure and control human personality may, indeed, be within the grasp of the psychologist. If so, the responsibility which this power entails must not be accepted lightly. Not only should the instruments of measurement be perfected and validated as well as may be before they are made available for general use, but their use should be limited to those who are properly trained to administer and interpret them. The scientific integrity of *homo psychologicus* is not alone adequate to this end. The public must be informed and educated to protect itself against the

[20] L. Burbank and Wilbur Hall, The harvest of the years, Boston & New York: Houghton Mifflin, 1931, p. 273.

incompetent and the dishonest. Our Association could well be the mouthpiece of its members in this important task.

These remarks upon the *homo psychologicus* and his relation to life can be briefly summarized in the statement that psychology is of necessity both a pure and applied science, and touches particularly closely upon every day life; that in recognition of this the American Psychological Association should depart from its traditional status in the following respects:

a. Define the *homo psychologicus.*
b. Propose minimum standards for his training.
c. Determine the training needed for at least the most active of the applications, and recommend the means for obtaining it.
d. Survey the possibilities for extending psychological service within the community, and thus provide employment for the growing army of psychologists.
e. By publicity devices or otherwise protect the properly trained psychologist from the inroads of the charlatan.
f. And finally, by some means exercise vigilance to the end that both the psychologist and the public may be protected from the effects of misinterpretation of scientifically sound conclusions.

These are indeed drastic proposals, but they are in the spirit of the times, and will, I hope, be sympathetically received by a fair minority of our membership. The American Psychological Association has had a glorious past and can be justly proud of its achievement in the upbuilding of academic psychology. Still greater honor will crown its efforts to link psychology and life.

ALFRED BINET'S CONCEPT OF INTELLIGENCE[1]

BY EDITH J. VARON

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Alfred Binet began his studies in psychology in 1880, at the age of twenty-three. He was a young student of the law who had read and been strongly influenced by the English associationists, and by Hippolyte Taine, whose 'De L'Intelligence' is in the nature of a general treatise on psychology.

From the beginning Binet concerned himself with fundamental problems concerning the nature of the mind. At first he tried merely to elaborate on the laws of association with the help of experiments performed under hypnosis. These studies led, however, to a repudiation of the laws of association as the ultimate explanation of mental phenomena; for Binet found cogent evidence that two or more consciousnesses (or personalities) may coexist in the same individual and may to a certain extent work together (*e.g.*, share certain memories); and the laws of association cannot explain how the gap between the two consciousnesses is bridged when it is, or why in other instances it is not bridged at all. They cannot explain why certain (especially recent) memories are called up, and why others completely fail to appear:

[1] This article was written at the suggestion of Professor Walter F. Dearborn of Harvard University, partly as a reply to certain criticisms of Professor Spearman (see 'Our need for some science in place of the word "intelligence,"' *J. Educ. Psychol.*, 1931, **22**, 401–410), but chiefly to bring to the fore some of the more generally neglected aspects of Binet's work. In neither Bertrand's 'Alfred Binet et son Oeuvre,' nor Martin's thesis on Binet (Robert Martin, Alfred Binet, thése complementaire présentée à la faculté des lettres de l'université de Paris, Paris: Les Presses Universitaires de France, 1924), does one find a single reference to Binet's last theoretical article, 'Qu'est-ce qu'une emotion? Qu'est-ce qu'un acte intellectuel?' without a knowledge of which it is impossible to understand the trend which Binet's thought was taking. The plan of the present article has been to present the most important quotations verbatim, to avoid losses due to lack of insight or errors of misinterpretation by the writer. Especially full quotations are given from the article just mentioned, since it is as important as it is little known.

. . . If, therefore, there is no response to this tie of resemblance, it must be because the play of association of ideas is not strong enough to arouse it, and association is therefore not sufficient to explain the development of the mental life. Undoubtedly something else besides these slight bonds is necessary to connect ideas. More profound causes, whose nature it is difficult to determine, just because they are unconscious, operate to apportion our ideas, perceptions, memories, and all our conscious states into free and independent syntheses. When we are in one of these syntheses we have difficulty in rousing an idea which belongs to a different synthesis. In general, association of ideas is not enough; but when the elements of this second synthesis have once been revived for one reason or another, then the entire synthesis reappears.[2]

Binet discarded associationism as the ultimate explanation of psychic phenomena because he had arrived about at the end of the then most promising path in psychology, and had found that as a matter of fact this path does not lead out of the wilderness. The explanation of psychic phenomena was almost as obscure as it had been before. And for the time being the understanding of these phenomena offered a most baffling problem, since there was now no tried and proved method of studying them.

Binet, deprived of his belief in associationism, was disoriented. He had no clear idea where to turn, what were the major problems of psychology, what theory would yield a valid interpretation of the known facts, or what method should be substituted for the one he was discarding. There is a period of about five years (1890–1895) during which he was evidently trying to find himself. He was not being very productive, but was critically studying the experimental methods, and through this study determining for himself what was the most outstanding problem of psychology, and what method of research would drive to the heart of it. We shall see that his conclusion was that psychology must concern itself first and foremost with the measurement of general intelligence.

Binet's frequent references to Taine show that he had been

[2] Alfred Binet, Alterations of personality, 1892, translated by H. G. Baldwin, p. 269.

strongly influenced by him. Quite possibly it was from Taine that Binet took his definition of intelligence. In the introduction to his 'De L'Intelligence' Taine writes:

> If I am not mistaken, we mean nowadays by Intelligence what was formerly called Understanding or Intellect—that is, the faculty of knowing; this, at least, is the sense in which I have taken the word.[3]

Thus, Taine included in intelligence almost all the phenomena studied by psychology; and Binet, both then and later, did the same.

At the same time that Binet through his studies of personality was led to discard associationism, he was beginning his studies of child psychology with his own children. In 1890 he published two reports of his observations; in the first of these he grazed the subject of intelligence; in the second he openly broached it.

He had measured the ability of his children to perceive differences in the size of two sectors of semi-circles, and found that Madeleine, age 4^{-3}, could perceive a difference in size when it amounted to as little as 3/40 of the smaller sector. The average adult can perceive a difference of 2/40. These facts suggested to Binet the desirability of comparing the perception of children with that of adults, but he stipulated that the value of the study would depend on the use of the *same* subjects, as they pass from childhood to adulthood, to see what had happened to the *individual's* perceptions. He continues:

> Until I am able to make this comparison, I take the average figure for the adult, which is equal to 2/40 of the smaller angle, and it is with this average figure that I allow myself to compare the results of the experiments which I have just summarized. It is, however, evident that the difference between 2/40 and 3/40 is altogether minimal, and that it could in no way express the relation which exists between the intelligence of an adult and that of a child. In the event that one should succeed in measuring intelligence, that is to say, reasoning, judgment, memory, the power of abstraction, which does not seem to me to be absolutely impossible, the

[3] Hippolyte Taine, De l'intelligence, Paris, 1870, p. 4.

figure which would represent the average intellectual development of an adult would represent an altogether different relation to the figure expressing the intellectual development of the child.[4]

This passage obviously presages Binet's subsequent work on intelligence. The idea of mental level or age is implicit in the 'figure' which is to represent intellectual development. Already, in 1890, he supposed that intelligence could be measured through the measurement of memory, reasoning, judgment.

If intelligence is as general and fundamental as Taine's definition implies, why should it be measured through these functions rather than through others? Possibly the choice was determined by the time at which different functions reach their full development:

. . . I limit myself to pointing out how interesting it would be to know whether, as my observations seem to indicate, the intellectual development begins with the lower functions, which may attain a high and almost complete degree of evolution at a time when the higher functions are still in a rudimentary state.[5]

Differences should be more apparent if the comparison of two individuals is based on a function which has not yet reached its full development in at least one of the individuals being compared, but is to some degree present in both. This principle is still employed in mental testing.

The sequel to this article, also published in 1890, is an avowed study of the development of intelligence. It opens with the first definition given by Binet, one which is not radically different from Taine's:

That which is called intelligence in the narrow sense of the word consists of two things: first, perceiving the external world; and then taking up these perceptions again in the memorial state, handling them again, and meditating on them.

We shall try, in the two studies which follow, to determine what there is that is peculiar to the perception and to the ideation of the

[4] Alfred Binet, La perception des couleurs et des nombres chez les enfants, *Rev. Phil.*, 1890, **30**, p. 74.

[5] Alfred Binet, *loc. cit.*

two children of two to five years who are subject to our observation.[6]

Intelligence is here defined in terms of perception, which may be identified with the broadest sense of the 'faculty of knowing'; but it is also defined in terms of the 'higher processes,' which operate upon these perceptions. With the aid of this complex definition, Binet set about studying the peculiarities of juvenile intelligence.

Many of the observations reported in this article found their place in the measuring scale for intelligence, *e. g.*, tests with pictures; designation of objects, of things; tests with numbers; comparison of lengths of two lines.

In 1892, light is thrown from a slightly different angle:

> . . . what we call our mind, our intellect, is a group of internal events, very numerous and very varied, and . . . the unity of our psychical being should not be sought elsewhere than in the arrangement, the synthesis—in a word, the *coordination* of all these incidents.[7]

In this passage, which definitely assumes that our 'minds' are somehow held together and made unitary, though composed of varied, apparently disunited events, we find a first attempt to account for the coexistence of a unified personality and apparently unrelated parts of that personality. It is a vague statement that is made here, especially in comparison with earlier assertions (as in 'The psychology of reasoning') which were quite definite. By this time Binet had concluded that ". . . we are obliged to be content with very vague notions, since, all things considered, they are worth more than false notions, and we do not hesitate to prefer them to physiological hypotheses, which while seeming more exact are really more hypothetical." [8] To understand Binet's work one must bear in mind this conviction of his; it grew stronger with time; it explains why in his later works the theoretical discussion becomes relatively sparse and incomplete, while

[6] Alfred Binet, Perceptions d'enfants, *Rev. Phil.*, 1890, 30, p. 582.

[7] Alfred Binet, Alterations of personality, 1892, tr. by Helen G. Baldwin, 1896, p. 350.

[8] Alfred Binet, Alterations of personality, Paris, 1892, p. 79.

the collection of facts increases in volume. He tried to avoid making *a priori* assertions.

There are signs that the suggestions contained in the passage above quoted were followed up in later works, as 'L'étude expérimentale de l'intelligence,' the articles on the insanities, and the 1911 article on emotions and intellectual acts, in which Binet investigated the integrative factors of the personality. He did not, however, live to complete this aspect of his research.

The term 'intelligence' was again explained, in a slightly different way, in 1894:

> From the very beginning of these studies, we cannot sufficiently insist on this idea that the sensation as a simple element is never realized alone in an adult person; it is for the purposes of study and analysis that one separates the sensation from all that which accompanies it; in reality, behind the sensation there is always the intelligence, as behind movement there is always the will.[9]

Binet has always spoken of 'l'intelligence,' *the* 'intelligence,' not *an* 'intelligence,' or merely 'intelligence'; he has evidently been referring to something unitary while defining it in terms of a number of factors. And in the above quotation he indicates in what respect it is unitary. It is that active agent by which we perceive, by which we know. The final inference one could draw from this passage is that intelligence is that by which we are conscious. 'Intelligence' becomes the broadest, most fundamental of psychological concepts. In this book on experimental psychology, no attempt is made to link intelligence with specific functions—or to separate it from them. The exact nature of the relationship is never made explicit by Binet. From this passage one might infer that it is causal; not so from the 1890 article. But some relationship is always implied—intelligence is never spoken of as a function which is quite independent of the more specific functions.

What is especially to be noted here is the basic nature of intelligence. For this statement was made in 1894, the year before Binet published his program for research in individual

[9] Alfred Binet, Introduction à la psychologie expérimentale, Paris, 1894, p. 25.

psychology. This program marked the turning point of his career, and it is well to understand how he happened to turn to the study of individual differences as he did—that is, through the measurement of intelligence.

In this same volume on experimental psychology we find a critical review of the methods of investigation then in use. None of them fully satisfied Binet. The fault of 'personal introspection' is that it leads to false generalizations; conclusions which are true for only a particular individual are assumed to be true for psychology as a whole. At the other extreme, highly objective experimentation, in which the observer and experimenter are in different rooms, or the observer is given his choice of a limited number of responses which the experimenter has chosen beforehand, also leads to erroneous conclusions. Binet suggested as a mean between the two extreme methods of pure introspection and of objective observation a method of 'comparative introspection.' By this method the conditions are still controlled, but the observer is allowed to report freely, and the experimenter does not suggest the form of his responses. But a number of observers are used, and only what is common to all of them may be retained as a truth in general psychology. That is, the trend of the argument in this book is in this direction: The facts which we now have in psychology are not valid either because of faulty experimentation, or because they are true only for a particular individual, and not necessarily for all individuals. Now, the obvious inference from this argument is that if the facts of general psychology are invalidated through the effect of individual differences, then general psychology cannot proceed further until it has studied the nature of these individual differences. And since there is nothing more fundamental, more essential to an individual than intelligence, what could be more logical than to begin the study of individual differences with an examination of intelligence?

The above description reflects the outcome of the years of groping between 1890 and 1894. In 1895 appeared the article

on 'Psychologie individuelle,' [10] which draws up an elaborate program for the experimental study of individual differences. The method proposed is correlational:

> The aim of individual psychology . . . is to study the different psychic processes of man. . . . It studies the properties of the psychic processes which vary from one individual to another; it must determine the variable properties and then study to what point and how these vary according to the individual. . . .
>
> One can, to begin with, distinguish two great problems:
>
> 1. To study how the psychic processes vary according to the individual, what the variable properties of these processes are, and to what point they vary.
>
> 2. To study the relations to each other in which the different mental processes are found in a single individual.[11]

Problem one is attacked by choosing two faculties, A and B, and inquiring whether, when A is well developed (in a group of individuals) B will also be well developed.

> It would be necessary, to be able to give an answer to this question, to study by how much *at least* faculty A must differ from the normal average for a difference to result for faculty B also.[12]

For problem two the methods are:

> 1. In a single individual one causes a variation of one mental process and one studies whether this variation induces with it changes in other processes of the same individual.
>
> 2. One chooses in advance a certain number of individuals, one then examines whether the individual differences for the processes go parallel to each other and correspond to each other in a regular manner; from this correspondence one can deduce the more or less intimate relation among the different processes.[13]

The objects of the psychology of individual differences were, even then, to determine norms for various specific processes; and also to determine the relationships existing among these processes in a particular individual. The out-

[10] Alfred Binet et Victor Henri, La psychologie individuelle, *L'année psychol.*, 1895, 2, pp. 411–465.

[11] Alfred Binet, *loc. cit.*, p. 412.

[12] Alfred Binet, *loc. cit.*, p. 420.

[13] Alfred Binet, *loc. cit.*, p. 422.

come of all this was to be tests which would serve to differentiate individuals one from another:

> One must investigate whether, with the knowledge and means which we now possess, we cannot already determine series of tests to be made on an individual to distinguish him from others, and to be able to deduce from the tests some general conclusions relative to certain habits and faculties of this individual.[14]

Among the processes named in this article as worthy of investigation for this end—memory, nature of mental images, imagination, attention, faculty of comprehension, suggestibility, æsthetic feeling, muscular strength and strength of will, and coup d'œil (observation)—intelligence is not included. There is no denying that the word 'intelligence' does not appear in this article. But then, why should it? We have already seen that intelligence is something which underlies almost all—probably all, without exception—of these processes; its connection with them is so intimate that to study them is to study it.

This apparently random conjecture of mine finds plentiful support. The first article on individual psychology had said nothing of how differences were to be measured. This deficiency was remedied by an article in the *Revue Philosophique* three years later. Here Binet makes it absolutely explicit that he is directing his efforts toward the measurement of intelligence:

> But if it is a matter of measuring the keenness of intelligence, where is the method of measurement? How to measure the richness of inspiration, the sureness of judgment, the subtlety of the mind? . . .
>
> I hasten to declare that I bring no precise solution to these problems; and I do not believe that it is possible at the present moment, when individual psychology is still in a state of infancy, to invent a satisfactory system of measurement; this system could be constructed only by virtue of *a priori* ideas, and it would not, probably, adjust itself to the immense variety of manifestations of intelligence. It is *a posteriori* that it will be necessary to proceed, after having gathered numerous facts.[15]

[14] Alfred Binet, *loc. cit.*, p. 434.

[15] Alfred Binet, La mesure en psychologie, *Rev. Phil.*, 1898, 46, pp. 8, 9.

This passage makes it perfectly clear that Binet wished to measure an *intelligence* which manifests itself in a *great number of ways;* to this extent this passage foreshadows the measuring scale of 1905.

His articles on experiments between 1895 and 1905 show that he was putting this idea into practice. He usually tried to find whether, when A (intelligence) is well developed (as determined by the student's standing at the head or foot of the class), faculty B—a specific kind of memory, ability to count little dots, etc.—will also be well developed. He was finding tests which correlate highly with intelligence, although they involved very different activities.

Yet he did not pretend that he was actually going to *measure* intelligence:

> . . . I have not sought, in the above lines, to sketch a method of measuring, in the physical sense of the word, but only a method of classification for individuals. The procedures which I have indicated will, if perfected, come to classify a person before or after such another person, or such another series of persons; but I do not believe that one may measure one of their intellectual aptitudes in the sense that one measures a length or a capacity. Thus, when a person studied can retain seven figures after a single audition, one can class him, from the point of view of his memory for figures, after the individual who retains eight figures under the same conditions, and before those who retain six. It is a classification, not a measurement. It is not at all the same thing as to measure three wood beams. In the latter case one really measures, one establishes, for example, that the difference between the first beam and the second is equal to the difference between the second beam and the third, and that this difference is equal to one meter. It is absolutely precise. But we cannot know, with respect to memory, if the difference between a memory for five figures and a memory for six figures is or is not equal to the difference between the memory for seven figures and the memory for eight figures; we do not know, moreover, what the value of this difference is; we do not measure, we classify.[16]

Binet's next important contribution to the literature on intelligence was 'L'étude expérimentale de l'intelligence,'

[16] Alfred Binet, *loc. cit.*, p. 122.

which appeared in 1903. Curiously enough, the word 'intelligence' appears nowhere in the book except in the title. Evidently Binet still did not consider it to be necessary to explain at length what he meant by intelligence, or how it is related to the various processes. But as the volume includes discussions of many of the tests suggested in the 1895 article, and new discussions on thought and imagery besides, these processes must constitute either elements or manifestations of intelligence. The study led, moreover, to the discovery of imageless thought, which later revolutionized Binet's theories.

The original purpose of the book seems to have been to "know the ruling faculty of the individual, and from it to deduce his entire mental organization." [17] In other words, he was looking for a qualitative, not a quantitative 'general factor'; the characteristic of an individual or of a type would be a general *form* of reaction. The book hints at the introvert-extrovert classification, though no such classification was found satisfactory. The general factor which Binet sought was one which would explain the emotional as well as the intellectual life. The book is another effort to reconcile the unity and variety which coexist within the individual. Plainly Binet did not suppose that the mind is merely an accumulation of unrelated 'faculties,' although, since he did find empirically that reasoning and memory, for instance, are not always found together, he concluded that these faculties are independent of each other. But we are entitled to suppose that he would have related both reasoning and memory to intelligence, though we cannot say exactly how.

Next after this book appeared the 1905 scale itself. There has been some speculation as to where Binet found the idea for the scale; he himself offers an answer. He introduces the scale by a discussion of one recently tried by his contemporary, Blin, and Damaye, a student of Blin's. This scale consisted of questions or tests on twenty 'themes.' Here are a few of these questions:

[17] Alfred Binet, L'étude expérimentale de l'intelligence, Paris, 1903, p. 303.

"What is your name?" . . . "When were you born?" . . . "Are your parents living?" . . . "What do they do?" . . . "Put out your tongue." . . . "Put your finger on your left eye." . . . "Go to the wall and come back here." . . . Experiment with little dots . . . Name objects shown—key, pen, pencil, etc. . . . "What color is this pencil?" . . . "Are you less thirsty when it is hot than when it is cold?" . . . "What time is it?" . . . "Is a week longer than a month?" . . . "Where are you, here?" . . . "Is Brittany in France?" . . . "What do soldiers have on their heads?" . . . Questions on reading, writing, spelling, and arithmetic . . . "What is the difference between the catholic religion and the protestant religion?" [18]

These excerpts from the twenty 'themes' show the variety Blin and Damaye had used in attempting to rate the intelligence of members of the colony at Vaucluse by getting a rough score of their achievement on these tests. They did not restrict themselves to a particular type of test, or to psychologically simple tests. These questions are largely informational; but, in addition, they do involve comprehension, judgment, and reasoning as well as memory. This is quite obvious in questions like those on thirst and religion.

Binet heartily commended this test of intelligence, although he criticised it because the scoring was entirely subjective and because the questions were arbitrarily chosen.

The exposition of the Binet-Simon 1905 scale for measuring intelligence follows immediately upon this discussion. The questions included in it—some of which, for instance differences, may actually have been borrowed from Blin and Damaye—were chosen and could be scored with some semblance of objectivity. It seems quite plausible that the immediate suggestion for the 1905 scale, which is really but a crystallization of Binet's work for years past, came from this scale of Blin and Damaye. He doesn't say so; he merely praises them and says that he and they are using the same method.

This, of course, is a purely contingent reason for putting the scale into this form; and one may well ask what is the

[18] Alfred Binet, Sur la necessité d'établir un diagnostic scientifique des états inferieur de l'intelligence, *L'année psychol.*, 1905, 11, p. 189.

relation between the form of the scale and Binet's theory of intelligence. We have already found cogent evidence to the effect that for Binet intelligence was something involving both unity and variety. In 1905 he reiterates that the simplest psychological act may involve intelligence; yet one need not put all psychology into tests to measure intelligence, one need only put into tests what is most important for such measurement.

The differential value of certain functions was noted in 1890, when it was also hinted that different functions might be of value for establishing the degree of intelligence at different levels of development. In 1905 this idea takes the form of stating that the function which differentiates between the normal and the feeble-minded is judgment, or common sense. This is not to say that a feeble-minded person develops normally until he reaches the point where judgment should begin to appear, and then suffers an arrest of development. Judgment, even as memory, may be present in varying degrees, the lesser degrees presumably being associated with lower levels of development. It should, therefore, be possible to find by means of the tests the degree to which judgment is present in an individual, and thus estimate the degree of his inferiority. But Binet leaves it an open question whether the same method suffices for an estimation of superiority or whether other characteristics besides judgment do not have to be tested for this purpose.

This gives us the following possible reasons why the 1905 scale took the form it did: (1) Binet had collected in the course of the previous ten years a large number of tests which he knew to be useful in differentiating the intelligent from the unintelligent; he may have reasoned that performance on a number of tests will tell more about an individual than does performance on a single test. (2) The form of the scale may have been immediately suggested by that of Blin and Damaye. (3) The heterogeneity of the scale may have been intended to give the subject a number of opportunities to show judgment in varying situations.

The descriptions accompanying the tests, telling what they

are intended to test, do confuse interpretation of his theory. They have even been taken to indicate a regression to the old faculty doctrine. But the tests are not really reduced to specific faculties; *e.g.*, of test 7 he says that its object is "to discover if associations exist between things and their names. Comprehension and the first possibilities of language are here studied. This test . . . represents the second degree of communication between individuals. . . ." [19] It seems probable that these descriptions were meant merely to differentiate one test from another, as a guarantee against duplication and also as a way of giving a picture of what the measuring scale was doing—to show how many and what general kinds of activity the tests sampled.

The scale, as Binet himself used it, did not constitute a final test of intelligence, intended only for use in classifying individuals. It was an instrument for further investigation into the nature of intelligence. In 1908, for example, he used the tests in establishing the mutual independence of intellectual level and emotional make-up; he likewise established that there is a positive relation between degree of intelligence and attention. The use of the tests was also preliminary to the formulation of his theory. The first step here was to classify the subjects according to their level of intelligence; the second, to examine their speech and find the peculiarities which it presented. Thus Binet found in the speech of the imbecile support for the theory of imageless thought. He found also that the kind of syntax used varies with the level of intelligence and at the same time serves to differentiate between individuals who otherwise appear to be of the same mental level—the feebleminded individual and the demented. This work culminated in a scheme which attributes three elements to thought: direction, adaptation, and criticism. This investigation, by means of the tests, into something other than just the measurement of intelligence, redounds to the advantage of research into the nature of intelligence. These three elements reappear among the four elements in intelligence, in 1909.

[19] A. Binet et Th. Simon, Méthodes nouvelles pour le diagnostic du niveau intellectuel des anormaux, *L'année psychol.*, 1905, **11**, 202.

The articles on the tests themselves give evidence that through the tests Binet was groping towards an understanding of intelligence. In 1909 he admits that the definition of intelligence

> is a problem of frightful complexity, and if we wanted to take it up in its entirety, we would be obliged to expound *a priori* views, the least danger of which would be to lead to distinctions and subdivisions which would seem important to us, but which might not be so at all. . . . Thus, we shall give no general theory of intelligence, but a detailed examination of special and ill known facts.[20]

And he makes a rough distinction between intelligence and scholastic aptitude (which is mostly memory) and another distinction between maturity and rectitude of intelligence, without saying exactly what intelligence is. But what should be noted is that Binet measured intelligence in order ultimately to know more about its nature.

Research into intelligence at the same time increased Binet's ignorance of what intelligence is, and clarified his conception of it. In 1909, in 'Les idees modernes sur les enfants' he proffered a tentative theory of the nature of intelligence. Having shown that the faculties are apparently independent, he continues:

> I have recently proposed, with Dr. Simon, a synthetic theory of the functioning of the mind, which . . . will show clearly that the mind is one, despite the multiplicity of its faculties; that it has one essential function to which all the others are subordinated; and one will understand better, after having seen this theory, what conditions the tests must fulfill in order to include all intelligence.
>
> In our opinion, intelligence, considered independently of phenomena of sensibility, emotion, and will, is above all a faculty of knowing (*connaissance*), which is directed toward the external world, and which labors to reconstruct it as a whole, by means of the small fragments of it which are given to us. What we perceive of it is element *a*, and all the very complicated work of our intelligence consists in uniting with this first element a second element, the element *b*. All knowing (*connaissance*) is thus essentially an

[20] A. Binet, Le développement de l'intelligence chez les enfants, *L'année psychol.*, 1908, 14, p. 74.

addition, a continuation, a synthesis, whether the addition takes place automatically as in external perception . . . or after a conscious search. . . . But note well that in this addition to the element *a*, there is already a host of faculties at work: comprehension, memory, imagination, judgment, and above all, speech. Let us retain only the most essential, and, since all this culminates in the invention of an element *b*, let us call the operation an *invention*, which is executed after a *comprehension*. The operation cannot be performed without our knowing what the question is, without our adopting a certain line, from which we do not deviate; thus a *direction* is necessary. . . . The ideas must be judged as fast as they are produced and rejected if they do not fit the end pursued; there must, thus, be a *censorship*. Comprehension, invention, direction, and censorship; intelligence is contained in these four words.[21]

Herein one sees that the associationism which formed the backbone of 'La psychologie du raisonnement' in 1886 has not been totally discarded; but it has been broadened by new concepts. At both times, in 1886 and in 1909, Binet attempted to show in a general way how the mind works. The new qualifications, invention, comprehension, direction and censorship, are not newly invented faculties of the mind which together constitute intelligence; they are descriptions of how memory, imagination, and other faculties work when intelligence is present. Furthermore, they are not hypothetical descriptions; they are the product of experimentation dating back to 1895 ('Memoire des mots,' and 'Memoire des phrases'), and they go little farther than do the actual reports of the subjects. The theory above quoted does not explain the mechanism of intelligence. Rather, it summarizes qualitative descriptions of the most important phenomena accompanying intelligent behavior.

Later experimental work led to no published revision of this statement. The 1911 articles on the scale and on teachers' judgments of intelligence did not go into the theoretical aspects, although there is evidence that Binet was still turning over in his mind explanations of psychological phenomena, as will be shown.

[21] Alfred Binet, Les idées modernes sur les enfants, Paris, 1909, pp. 117, 118.

Binet approached, also, the philosophical problem of the relation between mind and body. His discussion is not developed with the aid of experimental evidence, but the outcome is in harmony with the statements made in experimental articles. To state it briefly, he emerges with a theory, resembling Aristotle's, which is neither idealistic nor materialistic, but according to which consciousness is itself an act which 'illumines and reveals.' From this theory follow certain suggestions as to the nature of intelligence (to which, it must be said, Binet did not definitely subscribe, although he clearly favored them):

> From this we come quite naturally to see in the intelligence only an inactive consciousness; at one moment it apprehends an object, and it is a perception or an idea; at another time it perceives a connection, and it is a judgment; at yet another, it perceives connections between connections, and it is an act of reason. But however subtle the object it contemplates may become, it does not depart from its contemplative attitude, and cognition is but a consciousness.[22]

There is nothing in this statement that disagrees with the definition given in 1890. And these definitions are not compatible with definitions of intelligence or of the mind in terms of discrete faculties. One strongly suspects that when Binet speaks of 'faculties' it is a matter of convenience, instead of a lumbering and unclear statement in terms of manifestations of consciousness. If all faculties are nothing more than different manifestations of consciousness, there is no necessity for supposing that they are absolutely distinct from one another—as indeed they are not. This theory explains why it is necessary to measure intelligence by a number of complex activities, which cannot and need not be described in terms of specific functions. Binet found this theory to describe a very static kind of intelligence; and this, indeed, is the one respect in which it departs seriously from his other writings. Since the beginning of the century he had been pleading that due respect be paid to direction, censure,

[22] Alfred Binet, L'âme et le corps, 1906, edited by F. Legge, London: Kegan Paul, 1907, p. 117.

adaptation. At this time, in 1906, he left his description of intelligence in this unsatisfactory state only in order to avoid the difficulties of both materialism and idealism. Elsewhere, however, his descriptions of intelligence became more and more dynamic.

In the conclusion of 'L'âme et le corps' there is attempted a physiological explanation of consciousness—but of only one phase of it. The question is, why are we conscious of the outer world, and not of the physiological activities which cause consciousness? The answer offered is that events to which we are habituated—and physiological events are of course among these—tend to be eliminated from consciousness. But Binet did not go on to explain all that is happening when the intelligence contemplates, and "perceives connections between connections." In this book he deals only with the relation between mind and body; he disregards the specific bodily activities which produce specific consciousnesses.

It is rather surprising to find, after Binet himself advocated the use of correlational methods (1895) and even developed some himself, that he later used then very little. The reason is to be found in his epochal little book on experimental psychology (1894) where, while praising the more rigorous laboratory procedures, he vigorously protests against experiments in which the experimenter loses touch with the subject. No fineness of technique can do away with the need for introspective reports. In 'L'étude expérimentale de l'intelligence' (1903) [23] he repeats that although a number of subjects is necessary, yet, after the number has become too large to permit personal contact with the experimenter, one loses all that is learned from introspective reports, without making up the loss in any other way. He advocated the use of correlational procedures, therefore, only within limits.

Nowhere did he express himself more completely on this subject than in 'Les idées modernes sur les enfants,' (1909), where he says, in part:

> If ever a subject was debated, it is certainly that of the worth

[23] P. 299.

of correlations. Two absolutely contradictory opinions are current, and both confute the authority of proofs. According to one, which is upheld with ardor by the American Thorndike, the mind would be nothing but an absolutely heteroclite collection of faculties, which are as if juxtaposed, but which remain strictly independent. The contrary opinion, upheld by the American (*sic*) Spearmann (*sic*), with a great profusion of mathematical operations, is that the intelligence is one, that it exists in each one of us in a way deserving the name of general intelligence, and that one discovers a correspondence between the degrees of all our activities, even the most widely separated; there would be one, for example, between the ability to perceive sensations and the ability to manage one's affairs in life. It is the very opposite of Thorndike's opinion. All in all, these are extreme theses, and there are, as it were, intermediate truths which such controversies leave still standing. If one examines specifically the case of students and the aptitudes which they show for the different subjects which are taught to them, one can formulate on this subject divers observations which remain correct and established, no matter which extreme thesis one embraces. First of all, it is established that there is never an extremely low correlation. . . . Furthermore, there are no inverse correlations. . . . A final remark, the most important of all. There exists a faculty which acts in a way opposite to the aptitudes, it is general application to work. Whereas aptitudes give partial successes, general application to work exercises a levelling action and assures success in all branches which are attacked. It results from this that the effect of the aptitudes is seen less clearly when one has to deal with a group of very studious pupils; they replace ability by effort, and the calculations of theoreticians in search of correlations are thereby obscured.[24]

The controversy over the elements into which intelligence can be analyzed left him thus indifferent. There was no reason why it should affect him. As he said, he could go on establishing facts in spite of the controversy, and besides, his own method of studying intelligence was so different from the statisticians' that their quarrels concerned him only indirectly.

With this discussion Binet dropped the subject. His interest in quantitative studies was at all times secondary and

[24] Alfred Binet, Les idées modernes sur les enfants, Paris, 1909, pp. 242–3.

ancillary to his study of qualitative phenomena. Although he continued to revise the measuring scale, even as late as 1911, in order to perfect it, he was during this time making studies of a different nature which seemed to him more important than this revision. The fifteenth and sixteenth volumes of *L'année psychologique* included the beginnings of the results of five years of work by Binet and Simon on insanity and dementia. Binet evidently intended to carry this work much further. As he left it, it presents a description of the different types of mental alienation in terms of the dominant peculiarity of mental functioning.

The last, but very suggestive, indications of the tenor of Binet's thought are found in volume seventeen of that same journal. First there is a short preface, 'Le bilan de la psychologie en 1910,' in which he gives his view of the then current psychology. He sensed the importance which his measuring scale would have, and forecast the study of aptitude testing,—which he himself was about to undertake. For all this, he did not favor overemphasis on analysis:

> I hope in 1912 to be able to publish in *L'année* a study already begun a long time ago on the different aptitudes of children; it will be the logical complement of the measurement of intelligence. I think the knowledge of aptitudes is the finest problem of pedagogy. It has as yet been treated nowhere, at least to my knowledge, and at the moment we possess no sure procedure for investigating the aptitudes of any subject, child or adult. . . . There would be immense profit in knowing the worth of each one and the vocation for which his nature destined him; methods and tests which would throw light on vocations, aptitudes, and also inaptitudes, would render immeasurable service to all. As soon as the theoretical part of the problem shall have been solved, practical applications will not be delayed, and a whole intelligent organization of placement will be made, I know. . . . But the theoretical work to be done is immense. I am undertaking it now with a veritable army of tried collaborators.
>
> I have likewise in preparation . . . an essay on synthetic psychology. . . . The works which appear in psychology are especially analyses; these analyses no more allow one to form an idea of psychology as a mechanism than the crumbling of a statue

allows one to form an idea of the form of the intact statue. What one seeks to see, and what one by no means does see, in current treatises, is the ensemble, the synthesis; that is to say, the manner in which the machine works. I think it would be important to seek to understand how the different pieces of the machine exert their reciprocal action; that is what one can call synthetic psychology.[25]

Though the essay referred to was never published, we can at least guess the approach to the problem from a knowledge of the article in this volume, 'Qu'est-ce qu'une émotion? Qu'est-ce qu'un acte intellectuel?' The answer to these questions derives from Binet's own work on imageless thought, and that of the Würzburg school. Emotions and intellectual acts are attitudes; and a mental attitude is analogous to a physical attitude.

It is a preparation for the act, a sketch of the action which remains on the interior and is revealed to us by the subjective sensations which accompany it. . . .

. . . An attitude is composed of a series of intelligently coördinated acts with an end in view: but there are many ill-coördinated acts, and even there are many diffuse reactions. . . .

. . . the attitude is of an emotional nature when it is accompanied by strong sensations, of organic nature, and one will doubt the presence of the emotion so much less as the bodily effects are more intense. The emotion will thus be quite specially represented by this complex of organic sensations which accompany the attitude; thus are explained the three principle characteristics of emotion: 1. it appears especially bodily, since our attention is drawn to sensations having the body as seat and as stimulus; 2. it appears more personal than thought, since attention is drawn to us, to our body; 3. it appears affected especially by agreeable or disagreeable qualities, since that is the characteristic of organic sensations.

Inversely, we shall say that the attitude is of intellectual nature when it is accompanied by a minimum of subjective sensations, and a maximum of objective sensations and images; it is less bodily. . . . Also intellectual acts are phenomena which seem to us less personal, more general; and finally, they are colder, that is to say, removed from pleasure and pain.

[25] Alfred Binet, Le bilan de la psychologie en 1910, *L'année psychol.*, 1911, 17, pp. x, xi.

To pass from one process to the other, it suffices to change the balance of the organic sensations which accompany the attitude. Strip it of sensations, you have nothing but an intellectual act; add them, and you have an emotion. It is thus that the substantial unity of the mind is realized.

. . . It is . . . advantageous to consider the attitude as a variety of motor response, which distinguishes itself from others by the superior character of its organization; and one will say that the more this organization is realized, the more the attitude, all other things being equal, will be of an intellectual kind; the more, on the contrary, feeble the organization is, the more we will have to do with a phenomenon of emotion.[26]

Motor acts, emotions, and intellectual acts are thus shown to form a graded series. At the motor end of the series the reaction is relatively diffuse, and the attitude but crudely organized. To be exact, one cannot say that Binet himself included motor acts in the series, but the definition of attitude as a series of intelligently coördinated acts with an end in view allows one to do that. Not only is the motor attitude more crudely organized, but what sensations there are, are not organic, not objective or imaginal. When such objective sensations begin to enter into the attitude, and the attitude becomes more finely organized, if organic sensations are also involved, one has emotional attitudes. When the organic component of the attitude is eliminated, objective sensations and images become more important, and organization becomes still finer and stronger, then the attitude becomes an intellectual act. Thus Binet began to solve the problem which had long puzzled him, *i.e.* how it is that the mind, while appearing to be varied, still gives an impression of unity.

These reactions were not the only ones to be unified, however, but also the will and the unconscious were related to attitudes.

. . . for the voluntary character to appear, the attitude must be realized slowly . . . it should even have been in conflict with another different attitude, and it should triumph, and appear to be

[26] Alfred Binet, Qu'est-ce qu'une émotion? Qu'est-ce qu'un acte intellectuel?, *L'année psychol.*, 1911, 17, pp. 1–43.

chosen; it should be accompanied by a feeling, either relative or absolute, of satisfaction; the individual should realize well the consequences . . . and accept them; the attitude should appear reasonable, be judged useful to the individual, and conform to certain rules of good sense and morality, for in our conception of will there always enters a certain appreciation of value. It is by means of analogous considerations that one may distinguish between spontaneous and voluntary attention. . . .

In a word, the will is not a cause but an attribute of certain processes.[27]

It will be noted in the preceding passage that Binet indicated how this type of description might be carried over to other psychological phenomena. As to the unconscious, it is involved to some extent in every attitude, for research on attitudes has definitely shown how difficult it is to analyze attitudes. Analysis yields organic sensations; but, says Binet, it is not these organic sensations which would have explained the intimate act of thought; they would not have explained it better than mental images or internal speech. The unconscious involved in attitudes is different from the ordinary conception of the unconscious, as found in discussions of hysteria. It is rather akin to the unconsciousness of habit—which Binet had taken up in 'L'âme et le corps.'

Suppose that a series of attitudes is realized without images or sensations of any kind; we have seen that it is possible; in the absence of these elements of consciousness the attitude will not be felt, or noticed; but as it is connected with actions, as it is action in power, it will be able to give rise to acts and even to a complicated behavior. The transition from conscious to unconscious is here very easy to conceive; it takes place by a . . . loss of the accessory parts which normally accompany and enrich the attitude. The unconscious is the psycho-physiological process reduced to its physiological part, the attitude; or, to speak more exactly, the unconscious is the conservation and isolation of the motor part contained in every intellectual act; more briefly still, *the unconscious is a motor habit.* . . .

. . . In the most brilliantly conscious life there is found a . . . very important part of processes which remains unconscious by

[27] Alfred Binet, *loc. cit.*

nature: this is attitudes. To these attitudes are added conscious elements: these are sensations, images, and their derivatives. A complete process is thus composed normally of an unconscious part and of conscious parts. Without wishing to exalt one of these parts with respect to the others, we can admit that each can suffice and live an independent life.[28]

The implications for the pathological unconscious are not developed.

This article was published at what Binet rightly called the crisis of psychology—just before the reaction against associationism and introspection produced behaviorism and gestalt psychology. The intention was to indicate the trend which psychology ought in the future to follow:

It is thus necessary to introduce into psychology the notion of action . . . , of attitude, of movement; and here is the new fact, here is the indispensable addition. . . . The essential thing is to free oneself from the error which consists in seeing in mental mechanisms only states . . . which prevents one from conceiving the play of emotion, of the will, and even of thought; for these are acts and not states, and it is impossible to make one emerge from the other.

It is by this that the new theory breaks away from the sensationalist theory; it is a dynamism . . . [There is] an abyss which separates the two conceptions of mental life: the one, ancient, quite rational, which brings everywhere explanation, logic, continuity, which supposes that in mental life everything is explicable, is coördinated, can be justified; which accepts, for example, that reasoning is composed of premises and conclusions, and that one deduces from the premises, and one does not arrive at the conclusion without having gone through the premises. And in face of this theory appears the new one, a theory of action, according to which mental life is not at all a rational life, but a chaos of shadows crossed by flashes, something strange and especially discontinuous, which appeared continuous and rational only because . . . one described it in a language which brings order and clarity everywhere. But this is a factitious order, a verbal illusion, which no more resembles reality than the rumble of a classical tragedy resembles the unleashing of passions.[29]

[28] Alfred Binet, Qu'est-ce qu'une émotion? Qu'est-ce qu'un acte intellectuel?, *L'année psychol.*, 1911, 17, p. 42.

[29] A. Binet, *loc. cit.*, p. 47.

One may fairly deduce from this article that further descriptions of intelligence would have been in terms of attitude. Already the distinction between spontaneous and voluntary attention, which are related to different levels of intelligence, is described in these terms. So is 'will,' and that of course is involved in direction and censorship. Already in 1910 'disorganization' had been used to characterize dementia, and 'arrest of organization' to characterize feeble-mindedness. Perhaps ultimately Binet would have judged a person's intelligence by the fineness of organization, the complexity, and the flexibility of the attitudes of which he is capable. Aptitudes might also have been reduced to types of attitudes. It is not unlikely that Binet would have made further studies of attitude for the light that might thus have been thrown on the nature of the intelligence.

The graded series of attitudes enables one to understand why the feeble-minded, though incapable of fine coördination, are successful in industrial work. It explains why the intellectual level is lowered during strong emotion: by their very nature the two attitudes cannot coexist.

Binet died shortly after the publication of this article. According to his close collaborator, Simon, he used to say, "In four or five years, my task will be completed." Judging by his own sayings, we may suppose that this task was the formulation of a system of psychology which would bring together the phenomena of general psychology, individual differences, and the psychology of the abnormal.

The nature of intelligence would have furnished the materials for this system, since for Binet intelligence and psychology were coextensive. Because it did away with 'faculties' and association, and because it stressed the importance of organization of attitudes, this system would have borne some resemblance to the gestalt system.

Binet's theories bear some resemblance to Spearman's two-factor theory.[30] '*G*' may be thought of as the element which, being present in all acts, produces the 'unity' of the mind;

[30] C. Spearman, The abilities of man, 1927, p. 66.

the '*s*' factors account for variety. So far the two theories agree, and that is why the Binet-Simon measuring scale fits either one equally well. In the light of the evidence presented in the foregoing pages, there can be no doubt that the measuring scale does follow directly from Binet's theories, and is not, as has been claimed, incompatible with them.

The methods of investigation used by Binet and Spearman led them to very different conclusions. Spearman has used a statistical approach. Statistics are useful in describing distributions, and in showing to what extent a relationship holds between given variables. But descriptions of the nature of the variables, and descriptions of the kind of relationship holding between them—these lie outside the realm of statistics. For the statistician in psychology, therefore, it is necessary to postulate the factors between which he is going to find a relationship; and afterwards he will be able to say whether a relationship exists, and approximately how strong this relationship is—but how can he alter his preconceived notions concerning the qualities of these variables? It may help one to discover whether the factors are unique, general and specific, or grouped; but it does not tell us what these factors are. Statistics have helped to establish the theory of two factors, but have proved less helpful in demonstrating just what these factors are.

Binet, on the other hand, advocated and employed the method of comparative introspection, by which one not only obtains introspections, but also knows how much of the report is due to peculiarities of the individual. That there is a 'general intelligence' which manifests itself in specific ways, he accepted, though without stating it explicitly, in 1890. What he sought was an actual qualitative description of this intelligence. Experiments on memory, in 1894, yielded (through the reports of subjects) 'direction' as a factor. Observation of the ways in which the minds of the feeble-minded and of dements function led to 'comprehension,' 'invention,' and 'censorship.' At one time Binet tried to explain the unity of the mind as a form of reaction peculiar to the particular

individual;[31] in this he considered himself unsuccessful. The final revision of Binet's system followed, in 1911, upon further introspective studies; here attitudes and organization were made superior even to the four factors just named; indeed, there is the promise that attitude might have been used to describe the way in which they function.

[31] Alfred Binet, L'étude expérimentale de l'intelligence, Paris, 1903.

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THE STRUCTURE OF THE VISUAL WORLD. III. THE TENDENCY TOWARDS SIMPLIFICATION OF THE VISUAL FIELD

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1. Introductory Remarks

As the *Gestalt* psychologists have emphasized, our phenomenal field of vision is not an array of disconnected elements, but it contains wholes or units whose parts 'cling together.' Visual patterns may vary in their degree of wholeness or coherence. To describe the *opposite* of coherence one may use the term *disjunction*. Thus, a high degree of coherence is equivalent to a low degree of disjunction, and a low degree of coherence is equivalent to a high degree of disjunction.

The first paper of this series (**9**) set forth the theory that coherence and disjunction have a motor basis. Whether a pattern shall be perceived as coherent or as disjoined depends upon the nature of the oculomotor innervations that are aroused by the pattern. When there is a strong mutual resistance, or rivalry, between the fixation-tendencies due to the individual parts of the stimulus-constellation, the phenomenal pattern has a high degree of disjunction. When rivalry is weak, the phenomenal pattern is a relatively coherent unity.

According to the work of Wertheimer, coherence is favored by the following objective factors: (*a*) similarity between figure-elements with respect to color, etc., (*b*) continuity of the pattern, (*c*) spatial nearness of elements, and (*d*) monotony of formal structure. As Musatti has pointed out, all of these coherence-factors can be reduced to the single factor of homogeneity, or minimum differences.

Our theory assumes that the degree of motor rivalry de-

pends partly on subjective factors (age, attitude, etc.), and partly on objective factors. The chief objective factors that promote rivalry are differences of stimulation between the various parts of the field. When these differences become small, rivalry becomes weak, and therefore coherence becomes strong. It is in this way that our theory explains Musatti's law of coherence.

According to the argument of our second paper (**10**), motor processes not only directly determine the structure of perceptual consciousness, but they also influence perception in an indirect way, at the purely physiological level. Namely, the sensory and motor excitations of vision form an intercommunicating system, in which the sensory not only control the motor, but the motor can react upon the sensory. The spatial distribution of processes in the sensory brain-field is dependent, to a certain extent, upon the conditions on the motor side of the visual system.

The ideas developed in these two papers will now be applied to the explanation of a set of visual phenomena which may be grouped together as examples of an *assimilation-tendency*, or tendency towards simplification of the phenomenal field. I shall first review the experimental evidence in the literature which speaks for the existence of such a tendency. Following that, I shall consider some especially striking evidence which comes from a case of abnormal perception that I have recently studied. After summarizing the facts, I shall attempt to show that the assumptions of the motor theory offer a basis for an explanation of these facts.

2. Phenomena of Assimilation

(*a*) *Color-assimilation*—Under certain conditions the parts of a visual pattern appear alike in color even though they are objectively different (and are perceived as different under other conditions). This phenomenon was first systematically studied by Fuchs (**3**), and given the name of color-assimilation (*Farbenangleichung*). Fuchs showed that the effect depends upon the attitude of the observer. Perceiving the

differently colored parts as members of a coherent unity favors assimilation; perceiving the parts as a disjoined aggregate hinders assimilation. (Hereafter we shall speak of these two attitudes as the 'conjunctive' and the 'disjunctive' attitudes.)

An experiment by Koffka (**5**) furnishes a good example of color-assimilation, and of its dependence upon the mode of perception. This experiment employed a rotating disk composed partly of black and partly of white. The disk was so constructed that the proportion of black to white was greatest at the center, and decreased gradually from center to rim. The observer viewed a small portion of the disk through a rectangular aperture in a screen. If he regarded this rectangular area as a unitary whole, it appeared quite uniform throughout. If, however, he assumed an analytic, disjunctive attitude, he experienced a brilliance-gradient. If the area was divided into two portions by means of a fine wire stretched across the middle of the aperture, so that the observer experienced two separate unitary surfaces, each of these surfaces looked uniform but one of them appeared lighter than the other. Koffka showed that the apparent brightness of such a uniform-appearing surface is equal to the geometric mean brightness of its individual parts.

Fuchs (**2**) obtained especially striking assimilation-effects in subjects with hemiachromatopsia (total color-blindness in one half of the visual field). He found that a colored disk, tachistoscopically presented with its center on the boundary-line between the normal and color-blind halves of the field, might appear colored over its entire surface (although the same disk appeared gray when its whole area lay in the color-blind region).

In his experiments with normal subjects, Fuchs found that color-assimilation can take place not only between adjacent areas, but between spatially separate areas. Thus, if five dots are arranged in the form of a rectangular cross, and the central dot differs in color from the other four, it may take on the same color when the pattern is regarded as a coherent whole (**3**).

(*b*) *Confluence*—The phenomena of color-assimilation exemplify a tendency for visual patterns to simplify themselves, to become more monotonous or homogeneous in appearance. We shall consider now a second group of visual effects, which display a tendency for discontinuous patterns to appear continuous. We may speak of these phenomena as effects of *confluence*, or *assimilation of discontinuities*. They may be regarded as special cases of color-assimilation.

The first case of this sort was noted by Schumann (**12**). He found that many observers, when confronted by two segments of a straight line which are separated by a small gap, experience a 'subjective connecting line' between the two parts. Fuchs (**2**) has repeated the experiment, and reports that it succeeds only when the observer regards the pattern as a unitary whole. Thus the perceptual attitude exerts the same sort of influence as in the experiments on color-assimilation.

Schumann (**12**) also found that an outline-drawing of a square, with one side missing, was often perceptually filled in by the formation of a connecting line. Kroh, working under the direction of G. E. Müller (**8**, pp. 22 *ff.*), obtained this effect in 7 out of his 10 subjects. He also established the fact that the filling-in occurs only when the figure is apprehended as an incomplete square.

Lindemann (**6**), in his study of the apparent movements of briefly exposed patterns ('gamma movements'), observed certain characteristic tendencies towards the closure of gaps in incomplete figures. In the subjective filling-in of the blind-spot, Fuchs (**2**) finds other examples of a 'totalizing' tendency.

(*c*) *Spatial assimilation*—Let us turn next to the case of visual patterns which are made up of spatially separated parts, that is to say, patterns in which the parts are set off against each other by differences of spatial position. We have seen that there is a tendency for color-differences and discontinuities to disappear; is there also a tendency for *spatial differences* to vanish? Phenomena displaying such a tendency could be called instances of *spatial assimilation*.

Indications of this tendency are furnished by the effects of 'spatial displacement' (*Raumverlagerung*) which have been reported by Jaensch and his collaborators. These effects are said to be most pronounced in children of the eidetic and 'integrate' type. Jaensch's student Freiling gives the following example. A child was asked to attend to a vertical wire 50 cm. away; meanwhile a second wire, situated at one side, was gradually moved towards the first one. "In the case of Ernst H. one observed the striking fact that the two wires could be perceived as separate only when the lateral distance between them was at least 2 to 3 cm. If the distance was smaller, the two wires fused for the observer into a single wire. The same was later observed in the case of a number of other subjects. Maria L., when she observed two colored strings, experienced an apparent fusion in the following way. If, for example, a blue and a red string were presented side by side, eventually only *one* string was seen, and this appeared to be made up of alternate blue and red bits" (1, pp. 129 *f.*). Mayer (7, p. 84) obtained an analogous effect with a non-eidetic adult, when the experiment was carried out in reduced illumination.

It is reasonable to suppose that this spatial assimilation, like the other kinds of assimilation, is favored by the perception of the separated objects as members of a coherent unit or 'pair.' Perceptual coherence attains a greater strength in young children than in adults (*cf.* 9); and there are indications that coherence is especially strong in subjects of the type which Jaensch describes as 'integrate.' If the phenomenon of *Raumverlagerung* requires an exceptional degree of perceptual coherence, we can understand why it is obtained so much more readily by some subjects than by others.

(*d*) *The tendency towards monotonous form*—Geometrical forms can have different degrees of structural uniformity or monotony. A curved line is more uniform than a broken line, because it contains no places where the direction changes abruptly. A straight line is still more uniform than a curved line, because the direction is constant throughout. A closed figure is more uniform than an open figure, *i.e.*, than a figure

which contains an outstanding part in the form of a 'gap.' A circle is more uniform than any other closed figure, because the curvature of its contours is constant.

There is a tendency for geometrical figures to change, where possible, in the direction of greater *formal monotony* of structure. This might be called a tendency towards *formal assimilation*. Such a tendency would apparently correspond in part, though not entirely, to the *Tendenz zur Prägnanz der Gestalt* or *Tendenz zum Zustandekommen einfacher Gestalten* of Wertheimer, Koffka and Köhler.

As an instance of formal assimilation, we shall consider the tendency for broken lines to become phenomenally curved. We take the following example from F. B. Hofmann (4, p. 101). Let a broken line such as is shown in Fig. 1 be viewed under a

FIG. 1.

small visual angle, so that the break is just perceptible. If, now, a screen is placed in front of the object so as to block off all but a certain central portion, one no longer sees a broken line but a smoothly curved line.

Another observation by Hofmann (4, p. 97) shows that broken lines may become phenomenally straight. The two halves of a short vertical straight line are given a small horizontal displacement with respect to each other (Fig. 2), and the whole pattern is viewed under a small visual angle.

Provided the displacement is neither too large nor too small, one has the experience not of a broken line, nor of a straight vertical line, but of a straight line which is inclined at an angle. Under such conditions, the total pattern seems to distribute itself according to the principle of maximum possible regularity.

Another instance of the tendency towards monotonous form is the change of a closed figure in the direction of circu-

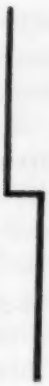

FIG. 2.

larity. In his study of 'gamma-movement,' Lindemann (6) tachistoscopically exposed a pattern of 11 dots arranged in circular form, together with a twelfth dot which was a short distance away from the circumference. This twelfth dot was observed to undergo a phenomenal displacement which brought it upon the circle-periphery.

Ruschmann (11) reports that, in the case of children of the eidetic type, visual figures tend to change their phenomenal form under the influence of a distracting stimulus (such as, *e.g.*, a sudden noise). These changes result in an increased regularity or symmetry. Thus, the Hofmann pattern of Fig. 1 changes to an arc or a straight line, and the pattern of Fig. 2 behaves similarly. Asymmetrical closed figures tend towards symmetry. It may be suggested (although Ruschmann offers a different explanation) that the distracting

stimulus produces this effect for the reason that it causes the children to see the figures in a less disjunctive, more coherent fashion than before.

3. Abnormal Assimilation-Phenomena: General Remarks

We have now seen that there are four groups of phenomena in normal perception which can be regarded as illustrations of a general tendency for the visual field to appear more monotonous—or less differentiated—that one would expect from the standpoint of the objective stimulus. Color-differences, discontinuities, position-differences and differences of geometrical form are all subject to this tendency towards levelling-out, or assimilation. Furthermore, we have noted that this assimilation-tendency is favored by the perception of the pattern as a coherent whole, and is opposed by the perception of the pattern as an array of disjoined parts.

We have next to consider a pathological case in which all the four types of assimilation are manifested in an extreme degree. This case not only furnishes very striking evidence for the existence of a general assimilation-tendency; it also shows, in a very clear-cut way, the dependence of the assimilation-effects upon the conjunctive and disjunctive modes of perception.

The subject, C., was first studied in 1931, by Drs. W. D. Turner and H. R. DeSilva and myself (see **13**, **14**). The results described in this paper are largely based upon a more extensive study, hitherto unpublished, which I was able to carry out in 1932–34.

C. displayed a number of singular visual abnormalities. One of them was an almost complete agnosia for colors; another, the Schumann phenomenon, has already been described in the preceding paper of this series (**10**). But the anomaly which is interesting from our present standpoint is one which may be called 'instability of the visual field.'

Visual patterns, when continuously fixated by this subject, tended very rapidly to lose their internal differentiation and

appear monotonous in structure. This loss of differentiation, or assimilation, displayed itself in four ways, corresponding to the four types of assimilation that we have discussed above.

These assimilation-effects were of central rather than ocular origin. This is most clearly shown by the fact that C. eventually *overcame* his 'instability' almost completely. The very fact that C. acquired a capacity to control the phenomena gave an insight into their origin. It proved that the instability of the visual field depended upon the fact that C. perceived visual patterns in an extremely *totalizing* fashion; for him the patterns had an abnormal degree of phenomenal coherence. He succeeded in conquering his defects, and maintaining stability of the field, by learning to *isolate* the details of a pattern from one another, that is to say, by learning to see the patterns in a more disjunctive fashion.

After C. had acquired this new mode of perception, he would sometimes revert to the old mode, especially when he was fatigued. He could also, at any time, deliberately assume his former attitude, whereupon pronounced assimilation-effects would occur. In such a case, he could even change back to the *disjunctive* attitude after the levelling process had already set in, with the result that the assimilation-phenomena would *reverse* themselves and the field regain its original differentiation.

C. is physiologically normal in color-sense, in visual acuity, in brilliance-discrimination, and in adaptation.[1] Under conditions where he is able to bring the disjunctive attitude to its maximum, his vision is as clear and stable as that of the average normal person.

4. Abnormal Assimilation-Phenomena

(*a*) *Color-assimilation*—C.'s visual acuity, when he wore glasses to correct his moderate refractive error, was a little better than 20/20 in each eye. This value was obtained with

[1] At the time of the earlier investigation, C. manifested an abnormally high light-threshold and an abnormally small visual field. These defects, which have now disappeared, can be attributed to the 'blurring-phenomenon' (see section 4*a*). C. has also succeeded in overcoming his color-agnosia.

the ordinary test-letters, which are black on a white ground. Despite his good acuity under these conditions, C., when he was first studied, displayed a remarkably inferior acuity under other conditions. This was especially the case when there was only a small brightness-difference between test-object and ground. For instance, when C. was shown a dark gray form, 10 cm. or more in breadth, on a black ground, he failed to see any figure at all at a distance of a few meters. He experienced nothing but a uniform black surface.

Even when a figure was clearly visible at first, it would, when continuously fixated, very rapidly blur away. In the extreme case, figure and ground blurred together into a perfectly uniform mass of color.

When C. gazed at a disk (25 cm. in diameter, and about 50 cm. away) which was half black and half white, the color of the whole disk became a uniform gray, after about 10 sec. of fixation. Unless the color-differences were comparatively weak, the blurring-effects required a definite *time* to develop. The smaller the color-difference, the shorter was the time, and the greater was the final degree of blurring.

The blurring of small figures was often accompanied by changes of form. Any contour-irregularities tended to round out, and the figure often assumed a definitely circular form before it disappeared. As soon as the contour had become somewhat hazy, the color of the figure began to grow more and more similar to that of the background. The figure might finally become indistinguishable from the background.

Sometimes, as in the case of a black cross, a sort of halo would develop outside the boundaries of the figure. Such haloes characteristically tended towards a circular form. The black cross eventually changed into a gray circle before it disappeared.

It is to be noted that these blurring-effects occurred only in the case of patterns confined to a single plane (or surface) in space. If a figure was placed a few centimeters *in front of* its background, there was no blurring. This fact in itself points against a peripheral explanation of the phenomena, and

suggests that they are dependent upon the presence or lack of sharp perceptual *disjunction* between figure and ground.

During the course of the preliminary study, Dr. Turner discovered the surprising fact that C. was able to control the blurring-effect voluntarily. Within limits, he could accelerate or check its speed as he chose. Meanwhile it was also found that the phenomena were becoming less and less pronounced as the experiments were continued. Thus, certain figures which, at the outset of the investigation, had blurred almost instantaneously for C., were eventually easily perceptible to him. It seemed, therefore, that he had somehow *learned* a way of improving the stability of his field.

When the second investigation of C.'s vision was begun (about 18 months after the end of the first), the blurring-effect had diminished radically. It no longer displayed itself spontaneously except under rather extreme conditions. Thus, it appeared that C. had developed an almost complete control over the effect, and that this control had now become habitual and effortless. However, he could always, if he so wished, relax this control and experience pronounced blurring-phenomena. Yet these 'willed' effects were seldom quite so extreme as those displayed during the earlier investigation.

C. was still unable to explain *how* he had managed to achieve this control. This remained a mystery until, after several months more, C. one day spontaneously discovered the solution.

Let us take a special example. If C. looked fixedly for a few seconds at a small gray dot on a black ground, with the intention of 'allowing the dot to blur,' then the dot would soon merge into the ground and disappear. What did it mean for him to relax his control and let the dot blur? It meant that he regarded the dot and its background as a single coherent *totality*. In his own words, it meant "thinking about the dot and its background at the same time." What did the 'control' consist in? It consisted in perceptually *isolating* the dot from its surroundings. In C.'s own words, again, it meant "thinking about the dot and forgetting about the background."

Suppose that C. regarded the dot and its background with the conjunctive attitude, and continued to fixate the dot until it had almost (but not quite) blurred away. If, now, he suddenly changed to the disjunctive attitude, the blurring ceased, the contours of the dot steadily regained their original sharpness, and finally the dot appeared almost as distinct as in the first moment of fixation.[2]

When C. looked at a page in a color-atlas, regarding the pattern as a single whole, the color of the small squares would spread into the white interspaces between the squares. If he regarded the group of squares and the system of interspaces as two separate patterns, there was no blurring. Analogous results were obtained with other figures.

Fuchs, in his experiments on normal color-assimilation, obtained this effect between spatially separated areas and not merely between contiguous areas. The same holds good for our subject C. He was shown three colored disks, each about 1 cm. in diameter, arranged in a horizontal line with interspaces of about 1 cm. In a typical experiment, the middle disk was orange and the other two were deep red. If C. fixated the middle one and meanwhile 'thought about' the other two, he reported that the orange color changed to red. This change took place without any blurring of contours; the disks remained sharp and separate throughout.

C.'s phenomena of color-assimilation seem to be broadly similar to those found in normal subjects, although much more extreme in degree. Especially striking is the fact that C. experiences not only an internal assimilation between the various parts of a unitary figure, but often a complete assimilation of figure and ground into a single uniform expanse of color.

(*b*) *Confluence*—Like color-assimilation, the phenomenon

[2] In another experiment, two dots were presented side by side upon the same background, and C. fixated the mid-point between them. He regarded one dot as *coherent* with the ground, the other as *disjoined* from the ground. After about 6 sec. of fixation, the first dot had blurred completely away, and yet the second dot, except for a slight haziness at the edges, remained quite distinct. This result shows that the production of blurring-effects cannot be attributed to a relaxation of ocular accommodation.

of confluence is displayed by C. in a very exaggerated form. For example, if C. looks at two black dots side by side, and regards them as 'belonging together' (*i.e.*, as members of a coherent 'pair'), the dots may appear to become joined together by a very distinct gray band or line. If he 'thinks of the dots as two separate things,' this line fails to appear. If the line has already appeared, it vanishes whenever C. suddenly changes to a more disjunctive attitude.

If a triangular pattern of dots is regarded as a unitary figure, the three dots appear to become joined by a continuous triangular contour-line. If the pattern is regarded not as a triangle but as 'one dot above and a pair of dots below,' only the members of the 'pair' become joined by a line. A quincunx-pattern, when viewed with different perceptual attitudes, gave several different distributions of connecting lines, as shown in Fig. 3.

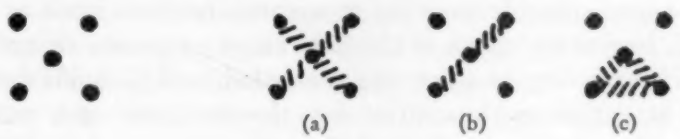

FIG. 3.

STIMULUS-PATTERN IS SHOWN AT THE LEFT, PHENOMENAL EFFECTS AT THE RIGHT.

Hence the confluence-effect, like the blurring-effect, is dependent upon perceptual coherence. It is easy to show, by the use of appropriate patterns, that confluence is favored by similarity of color, by spatial proximity and by monotony of formal arrangement of the elements. Thus the phenomenon can be made to furnish a very striking illustration of Wertheimer's principles of coherence (section 1).

With other sorts of patterns, the confluence-effect might involve a surface instead of being confined to a line. Thus, a small outline-drawing of a circle became filled in so as to form a continuous disk. A larger circle became filled in only partially. A pair of parallel lines was transformed into a solid rectangle.

If the contours of the objects are too sharp, confluence does not take place until they have been rendered slightly

diffuse by a brief period of fixation. As in the case of the 'blurring-phenomena,' a sharp contour seems to be a place of resistance towards the spreading of color.

If two dots are placed horizontally, the joining line forms more readily than when the dots are placed vertically.

Like the blurring-effect, the confluence-effect is now under C.'s control; pronounced examples do not now occur except under unusual conditions.

(*c*) *Spatial assimilation*—Under certain circumstances the confluence-phenomenon is replaced by one of *apparent movement.* If two dots are regarded as a coherent pair, then after a few seconds of fixation they may apparently begin to move towards one another and finally coincide as a single dot.[3] Movement occurs only when the elements are regarded as members of a whole. If two dots have started to move together—and even if they have already moved together and come into coincidence—the movement reverses itself as soon as C. begins to 'think of the dots as two separate things.'

If three dots in a row are presented, and C. holds fixation and attention on the central dot, the two outer ones move to coincidence with this fixated element.

The elements of a triangular dot-figure tend to remain stationary. At one time, however, C. experienced the following effect. When the center of the figure was fixated, all three dots moved to the center and coincided as a single dot. Movement could always be obtained if the figure was regarded not as a triangle but as made up of two parts—a single dot above and a unitary pair below. In this case the movement could take either of two forms. The members of the pair might move together horizontally to form one dot, or the dot above and the pair below might move vertically and approach each other.

Movement resembles confluence in the following respects. It is hindered by too sharp contours. (If the contours are

[3] At one time a pair of objects separated by as much as 20° would move to coincidence. The speed of movement increases as the initial separation of the objects is lessened; when their separation is very small, there may be simply a sudden change of apparent position without any perceived movement. Movement occurs equally readily in monocular and binocular vision.

very sharp, the figure must be fixated for a brief period before movement will begin. The objects do not, however, have to be fixated until they lose or change their shape.) Movement is more easily elicited in the horizontal than in the vertical direction. Finally, there is no movement except when the elements are perceived as members of a coherent whole.

Movement is evidently closely related to confluence, so that both phenomena can be regarded as two different expressions of a single tendency. It is often possible, by a slight variation in the stimulus or in the subject's attitude, to obtain either phenomenon from the same pattern.

Movement is likely to occur when the *coherence* of a pair of objects is especially strong. If two dots are very close together, and are treated as a coherent pair, movement tends to occur almost instantaneously. If C. deliberately inhibits this movement by adopting a more disjunctive attitude, the dots lose their apparent separation in a different way—by forming a connecting line. If the phenomenal disjunction is made still stronger, this line in turn disappears.

Two dots which are separated by a larger distance are more likely to acquire a connecting line than to move. But if, by an appropriate effort, C. increases their phenomenal coherence, then the line vanishes and the dots move together.

Thus movement and confluence are intimately related, and confluence in turn is hardly to be sharply separated from the blurring-effect, or phenomenon of color-assimilation. Movement moreover eventually came under the subject's control, and could be accelerated or checked by his deliberate attitude just as was the case with the other two phenomena. All three effects, therefore, can be regarded as so many special instances of a general principle of assimilation.

(*d*) *The tendency towards monotonous form*—The phenomena of blurring and confluence that we have already described furnish some examples of the tendency towards monotonous form. Thus, geometrical figures often approached a circular shape as they blurred.

The following experiment furnishes a still more striking example of the tendency towards monotonous form. C. was

shown a circle outlined in dots with one dot misplaced. (The time of exposure was not limited, as it was in Lindemann's tachistoscopic experiments.) This pattern appeared to C. as a perfect circle. He did not know that the circle was objectively imperfect until he was told this fact; and even then, he could not *see* any displacement except when he fixated the neighborhood of the irregular place. (Control-tests showed that this result could not be attributed to any indistinctness in his perception of the figure.) After a brief period of fixation, the dots became connected together by a circular contour-line.

5. The Significance of the Assimilation-Phenomena

It is now evident that our abnormal subject displays, in extreme form, the four types of assimilation which have been demonstrated in normal subjects. Furthermore, these abnormal phenomena lend support to our assumption that all four kinds of assimilation are related to each other, that they are simply different manifestations of a single tendency towards homogeneity of the visual field. In both normal and abnormal phenomena, again, we find that assimilation is favored by perceptual coherence, and opposed by perceptual disjunction.

Furthermore assimilation is not only *favored by* coherence, but it also tends to *produce* coherence. Assimilation makes the visual field more monotonous, or less differentiated; it levels out color-differences, discontinuities, spatial separations, and differences of form. And coherence itself, as we noted in section 1, is promoted by any decrease in the differentiation of the field with respect to color, space and form. Thus assimilation has the effect of increasing the coherence of the phenomenal field.

When coherence is already strong, then the assimilation-tendency itself has an unusual strength. Hence a field which is already highly coherent easily becomes still more coherent, through a levelling-out of the differences between the various parts of the field. In the extreme case of our abnormal subject C., coherence is so strong that the visual pattern may

degenerate into structureless, or almost structureless monotony.

So we see that coherence is accompanied by forces of destruction. Moreover, we are led to the inference that the normal stability of the visual field is dependent upon the normal mode of perceptual response. The normal field is (relatively) stable because of its lack of coherence, or because of its high degree of disjunction. Its stability cannot be guaranteed by purely anatomical arrangements, but must be intimately dependent on *the functional activities underlying disjunctive perception.*

6. Coherence and Disjunction; the 'Attunement' of the Motor System

Now, our theory holds that the activities directly underlying disjunctive perception are motor processes (see section 1, above). Disjunction increases, and coherence decreases, with the degree of rivalry, or competition, between ocular fixation-impulses. We shall briefly review some of the relevant ideas of this motor theory.

Let us consider, to begin with, the dependence of rivalry upon objective conditions. Rivalry is at its minimum in the case of a visual field which is perfectly homogeneous in color. Here there are no contrasting details in the field which could arouse a strong conflict of fixation-impulses. On the phenomenal side, the coherence of the field is as high as it possibly can be.

Rivalry is favored by contrasts or differences between one part of the field and another. If we introduce at some place in our homogeneous field a figure of unlike color, there arises a struggle of fixation-impulses between figure and ground Correspondingly, the seen field is now divided into two parts which are sharply disjoined from one another—a phenomenal 'figure' and a phenomenal 'ground.'

Although the figure as a whole is disjoined from the ground, its individual parts, if they are exactly alike in color, will have little tendency to enter into a motor struggle with each other. Therefore the figure will appear to the observer

as an *internally coherent* whole. But assume that the figure is made up of dissimilarly colored elements. This objective differentiation of the figure will promote an internal rivalry of fixation-impulses between the elements. On this account, the figure will appear less coherent, or more disjoined, than one of uniform color.

Not only color-differences, but spatial separations and differences of geometrical form between the elements can produce rivalry, and diminish coherence, in proportion to the magnitude of the differences.

When rivalry is strong, the eye is constantly impelled to fixate now one part of the field, and now another. It can find no stable position of rest. As rivalry becomes weak, the motor system approaches a state of equilibrium.

The degree of rivalry is lowest, or the state of complete equilibrium is most closely approached, when the field is perfectly homogeneous in color. In this case the eye is free to wander as it will; any position of the fixation-point is a position of relatively stable rest.

Let us again suppose, however, that the field is not homogeneous but contains a figure upon a ground. If the figure as a whole is sharply set off from its ground, the parts of the figure will arouse strong fixation-impulses. But if the figure has high coherence these impulses will not have a strong tendency to struggle against each other. Instead, they will tend to combine into a single resultant, or 'totalizing fixation-response.' This resultant will be directed towards the 'center of balance' of the figure (see **9**, sections 13 and 14).

This center of balance is a position of greatest equilibrium, or of maximum stability of fixation. Equilibrium will be less perfect than in the case of a homogenous field, because there is always a struggle of fixation-impulses between figure and ground. Thus the eye, even when it fixates the center of balance, will be under a constant impulsion to transfer its fixation away from the figure and towards the ground.

Rivalry is never completely absent. With any figure, there will only be a partial, not a complete combination of forces into a single resultant. There will always be a certain

degree of internal rivalry between the individual figure-elements acting as separate units. A figure is internally coherent when there is a *predominance* of the totalizing response; it is internally disjoined when there is a predominance of rivalry.

The relative strength of the two kinds of response depends not only upon the stimulus-pattern but upon a subjective or personal factor which may be called the 'attunement' of the motor system. Two persons, viewing the same objective figure, may have phenomenal impressions that differ in coherence. For instance, the young child's impressions tend to be more coherent than those of the adult. This difference, according to our theory, is based upon a difference in the properties of the two motor systems, or, as we shall say, upon a difference of motor 'attunement.'

We can describe the attunement in the case of the child by saying that the motor system has a pronounced tendency to attain *equilibrium* between the impulses due to the different elements of a visual figure. With the adult this equilibrium-tendency is weaker.

In order to experience a figure as disjoined, the child has to exert a special effort. That is, he must voluntarily bring an influence into play which reinforces the weak rivalry-tendency, and which opposes the strong combination-tendency.

The motor system of the adult, however, has undergone a process of training which has changed its original attunement. In his case, the motor impulses due to the parts of the pattern have a stronger tendency to struggle against each other, and a weaker tendency to combine into a resultant. If the adult wishes to have a perception which is more coherent—that is, more similar to the child's perception—he has to assume a conjunctive attitude. This conjunctive attitude means the introduction of an outside force which opposes the spontaneous tendency towards rivalry, and which favors the naturally weak tendency towards combination.

Thus the motor system of any individual has a certain natural tendency towards equilibrium. This attunement,

however, is not rigidly fixed, but can be temporarily modified by voluntary influences. In a given individual, as we have indicated, the attunement undergoes a progressive change with age.

7. Assimilation as an Expression of the Tendency towards Motor Equilibrium

The phenomena of assimilation represent a tendency towards the levelling-out of differences between the various parts of the sensory excitation-pattern. Now, it is at once obvious that such a levelling process must, on our assumptions, diminish the degree of motor rivalry. We now propose the hypothesis that assimilation takes place *because* it diminishes rivalry.

We have already suggested, in a previous paper (**10**), that the sensory part of the visual system is not independent of the motor part, but that the motor excitations are capable of reacting upon the sensory and changing their spatial distribution. This idea now enters into our explanation of the assimilation-tendency.

The motor system, as was indicated in the last section, has a tendency towards equilibrium. This tendency varies in strength from one individual to another, but it is never completely absent. Sensory excitation-differences *oppose* equilibrium, and the greater the differences, the stronger the opposition. Now, our hypothesis declares that the motor system can react upon the sensory excitations in such a way as to level out, wholly or partially, those *differences* which oppose the tendency to equilibrium.

If the stimulation-differences and the corresponding central excitation-differences are small, this levelling process may go to completion. If the stimulation-differences are large, the degree of assimilation will usually be relatively small or even negligible. That is, the tendency towards equilibrium is not ordinarily strong enough to level out large differences.

The amount of assimilation will depend upon the attunement of the motor system of the given percipient at the given time. When the attunement is such that there is a strong

tendency towards motor equilibrium, assimilation-effects will be pronounced. When the tendency towards equilibrium is weak, then assimilation will be correspondingly slight.

When one assumes the conjunctive attitude, assimilation is increased, because one has momentarily increased the equilibrium-tendency. The disjunctive attitude decreases assimilation, because it represents a weakening of the equilibrium-tendency.

If our assumptions are correct, assimilation-effects should be stronger in young children than in adults; and the facts seem to bear out this implication (although further experimental study of this point is needed).

In section 5 the assimilation-tendency was described as a tendency towards change of the visual pattern in the direction of increased perceptual coherence; and it also was pointed out that coherence, when it is already strong, tends to become still stronger through the action of assimilation. It is now evident that our theory provides a functional expression for these descriptive principles.

8. Application of the Theory to the Case of the Subject C

According to our interpretation, the abnormal assimilation-phenomena displayed by the subject C. were due to a single basic defect, namely an abnormal attunement of the oculomotor system. This defect can be described in two different ways, which are exactly equivalent to each other. We can say that there was *an abnormal weakness in the degree of rivalry*, or that there was *an abnormal strength of the tendency towards equilibrium*. The improvement of perception which eventually occurred can be ascribed to the development of an attunement more closely resembling that of the normal adult.

It has unfortunately not been possible to test the validity of this hypothesis in a direct way (by a photographic study of the subject's eye-movements). But one suggestive fact has already been mentioned in the preceding paper. C. possesses an unusual ability to maintain steady fixation upon a central object and at the same time attend to an object in the periph-

eral field. This fact makes it appear that the conflict of motor impulses, which normally produces a strong tendency towards fluctuation of fixation, must be much weaker in his case than in that of the normal subject. (In this way we interpreted the fact that C. obtains the Schumann phenomenon (**10**, section 3) in a situation where the normal subject does not.) [4]

Two dots which are placed horizontally show a stronger tendency towards confluence and movement, in C.'s perception, than two dots which are placed vertically. If our theory is right, oculomotor rivalry must be stronger in the vertical than in the horizontal direction. This assumption will also explain why C. obtained the Schumann phenomenon more easily in the horizontal direction than in the vertical.

In conclusion, we have arrived at a theory which accounts for normal and abnormal assimilation-phenomena, and for all the four types of these phenomena, by means of a single principle. This theory explains the dependence of assimilation upon phenomenal coherence, and is in fact integrally connected with a theory regarding the nature of coherence. An underlying assumption, namely that there can be a reaction of motor upon sensory processes, is, as a previous paper attempted to show, not without support. Finally, although our theory of assimilation still lacks direct evidence in its favor, it seems to provide the simplest possible explanation of the facts at hand.

9. Summary

1. There are four groups of phenomena in normal perception which illustrate a general tendency for the visual field to appear more monotonous, or less differentiated, than one would expect from the standpoint of the objective stimulus. These phenomena of levelling or assimilation include (*a*) the assimilation of color-differences, (*b*) confluence, or the assimilation of discontinuities, (*c*) the assimilation of differences

[4] It should be remarked, moreover, that the assimilation-phenomena described above were usually contingent upon very steady fixation. For example, when two objects had begun to execute apparent movement toward each other, a slight wavering of fixation was sufficient to check the apparent movement and make it reverse itself. Nevertheless, C. was able to maintain such a precise fixation that the results of repeated experiments show an excellent qualitative and quantitative consistency.

in spatial position, and (*d*) the assimilation of differences in geometrical form. These phenomena are favored by the perception of a visual pattern as a coherent whole, and are opposed by the perception of the pattern as an array of disjoined parts.

2. In an abnormal case described here, all four types of assimilation were manifested in an exaggerated form. The abnormality could be traced to the fact that the subject perceived visual patterns in an extremely *totalizing* fashion. He finally succeeded in conquering his defects, and maintaining stability of his visual field, by learning to see the patterns in a more disjunctive manner. This case furnishes striking corroboration to the conclusions drawn from normal phenomena. We are led to the inference that the stability of the normal visual field is intimately dependent upon the functional activities underlying disjunctive perception.

3. According to the motor theory of perception advanced in a previous paper, disjunctive perception corresponds to a strong rivalry, or competition, between ocular fixation-impulses. Coherence, the opposite of disjunction, corresponds to a weak rivalry—in other words, to a strong tendency towards motor equilibrium.

Rivalry is favored by *differences* between the several parts of the stimulus-pattern. The degree of rivalry also depends upon the individual 'attunement' of the motor system. The attunement of the motor system of a given person at a given time is characterized by a certain strength of the tendency towards equilibrium.

4. In another earlier paper, arguments were advanced for the idea that the motor processes of vision can react upon the sensory processes and change their spatial distribution. This idea is now made the basis for a theory of assimilation. This theory holds that assimilation is an expression of the tendency towards motor equilibrium. The motor processes react upon the sensory in such a way as to level out, wholly or partially, those *differences* of excitation which oppose the tendency to equilibrium.

5. According to this theory, the degree of assimilation

increases with the degree of phenomenal coherence because coherence is proportional to the strength of the equilibrium-tendency. Individual differences in the attunement of the motor system are accompanied by individual differences in assimilation-effects.

6. The pronounced assimilation-effects experienced by our abnormal subject are referred to an unusual attunement of the motor system. It is assumed that there was an abnormal weakness of rivalry, *i.e.*, an abnormal strength of the tendency towards equilibrium. The improvement of perception which eventually occurred is attributed to the development of an attunement more closely resembling that of the normal adult.

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THE LAW OF DISUSE

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The implications of the law of exercise are so numerous and so important for the whole of psychology that any evidence bearing upon it at once achieves special significance. The law of exercise, as stated by Thorndike, comprises the laws of *Use* and *Disuse*. The evidence to be considered in the present paper relates specifically to the law of disuse. "When a modifiable connection is *not* made between a situation and a response during a length of time, that connection's strength is decreased" (**8**). Since the evidence derives from a field of psychology not generally drawn upon for theories of learning and memory, a word of explanation is in order.

When, in a psychophysical experiment, the second of two stimuli is compared with the first, the judgment, at the time of its formulation, is partially determined by an event which, as stimulus, is over and gone. One of the immediate conditions of the judgment must therefore be the physiological after-effect or *trace* of the first, *i.e.*, the standard, stimulus. If, as is very frequently the case, the standard stimulus is compared with a series of comparison-stimuli symmetrically distributed about the standard, it is reasonable to suppose that judgments of 'greater' and 'less' will occur with about equal frequency. Any significant departure from such an expected frequency may then be taken as an indication of a change in level of the trace of the standard stimulus. If, *e.g.*, the standard stimulus is 100 gr., and the comparison-stimuli are 108, 104, 100, 96, and 92, one would be justified in expecting approximately an equal number of judgments 'greater' and 'less.' By varying the interval of time between the standard and comparison, a Method of Delayed Comparison can be put to the task of following the career of the trace of

the standard stimulus as a function of time. During the interval of time between the first and second stimulus, the trace of the standard is not being 'used' (*i.e.*, reinforced or repeated). The properties of disuse may therefore be inferred from the manner in which the frequencies of the major categories of judgment are distributed.

Ever since the earliest days of psychophysics it has been a well-known fact that judgments upon symmetrically arranged comparison-stimuli are not equally distributed. There is a marked preponderance of judgments in the direction of increase. In the case of the weights, *e.g.*, cited above, there would be many more judgments of 'heavier' than of 'lighter.' This departure from expectation was accounted for by Fechner in terms of a fading or weakening image of the standard. Since the judgment is made after the standard has gone, he supposed that it must be mediated by an image of the standard and that furthermore the image must become progressively weaker with the passage of time. In this way the preponderance of 'greater' judgments on the comparison-stimuli would be explained. This explanation Köhler has recently reformulated and elaborated in terms of a sinking physiological trace of the standard (**4**).

The obvious similarity between these views and the law of disuse has not often been noted. In a psychophysical context the law of disuse could read: when the trace of the standard stimulus is not reinforced or interfered with during a length of time, the strength of that trace is decreased.

As in the general field of memory, so in psychophysics, the view that traces or connections weaken with time has lately been subjected to much adverse criticism. It is argued that nothing can happen merely as a function of time. "In scientific descriptions of nature time itself is not employed as a causative factor nor is passive decay with time ever found. In time iron, when unused, may rust, but oxidation, not time, is responsible. In time organisms grow old, but time enters only as a logical framework in which complex biochemical processes go their ways" (**6**). If a trace appears to lose strength or effectiveness with time, the change must be due to interaction between traces.

The work on retroactive inhibition has lent powerful support to such a view, with the result that opinion is veering strongly toward some principle of interaction, in place of that of disuse, as basic in explanations of mnemonic changes. Woodworth argues that mere disuse or inactivity can not be supposed to have any effect on traces and suggests "that it is the interference of one activity with another which causes forgetting" (9). Jenkins and Dallenbach, as the result of their work on obliviscence during sleep and waking, conclude that "the results of our study as a whole indicate that forgetting is not so much a matter of decay of old impressions and associations as it is a matter of interference, inhibition, or obliteration of the old by the new" (3). Hunter finds this view in harmony with his own (2), as do also McGeoch (6) and a number of others.

In psychophysics the use of an interpolated stimulus between the standard and comparison has led to results in many respects similar to those secured in the more familiar studies of retroaction. In outline, the procedures, as well as the hypotheses which dictate them, are very simple. If between the first and second stimuli, a third stimulus, very appreciably more or less intense than the standard, is interpolated or used as a background, the assumption is that the traces of the standard and interpolated stimuli will interact, thus producing assimilation of the standard trace upward in the case of the more intense interpolation, and downward for the less intense. Lauenstein (5) and the present writer (7) have used these procedures, and have secured results exactly comparable, except in one important respect to be noted presently. The results of both experiments, however, do clearly support the assumption that the trace of the standard assimilates upward when interpolation is above the standard, and downward when it is below.

Thus it is seen that psychophysical judgments on successive stimuli produce results which, within a shorter temporal range, are in many ways similar to those secured in work on retroaction. And the attempted explanations of the results in the two fields are practically identical. One view has it

that the physiological change which occurs between initial stimulation and subsequent judgment or recall is due to weakening of the trace through disuse; the other maintains that the change is caused by interaction of traces. At the present time the first of these views seems to be receding in favor of the second. The purpose of this paper is to demonstrate that neither view alone is adequate.

It is suggested, furthermore, that the psychophysical Method of Delayed Comparison offers a more decisive test of the validity of the hypotheses in question. If one works with judgments of intensity, any change in trace-level is confined to the directions of increase or decrease, whereas changes in the traces of nonsense syllables, meaningful words, poetry, and the like, take place in numerous indeterminate ways, with the result that one can discover only that their effectiveness for recall has been altered. What specific changes have altered the effectiveness of the traces it is impossible to say.

In the usual psychophysical arrangement for successive comparisons, provided the comparison-stimuli are symmetrically distributed about the standard, there is practically always a preponderance of judgments in the direction of increase, indicating a weakening of the trace of the standard. The course of this weakening with time (after the initial positive time-error) is shown in Fig. 1. In Table I the sort

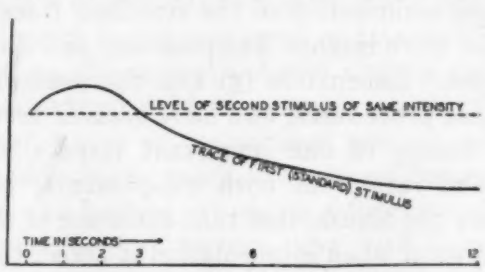

FIG. 1. After Koehler (4).

of results which have led to the assumption of a lowered standard trace may be seen in the first column. The negative values reveal a preponderance of 'louder' judgments.

The experiments reported in Table I were made upon

TABLE I

Values of time-error ('less' minus 'greater' judgments divided by total number) for two series of judgments on auditory intensities. Normal series includes standard and comparison stimuli. Second series involves interpolation of a third (soft) stimulus.

Os	Normal	Soft
G.	−0.14	−0.19
H.	−0.16	−0.52
F.	−0.09	−0.53
Ave.:	−0.13	−0.41

intensities of sound produced by a sound-pendulum. Five symmetrically distributed stimuli were compared after an interval of 7 sec. with a standard at 45°. Fifty judgments on each stimulus were secured from each of three observers. All the data here reported are thrown by a simple device into a single form. Judgments in the direction of increase (in this case 'louder') are subtracted from those in the opposite direction ('softer') and divided by the total number. A negative value indicates a sinking of the level of the trace of the standard stimulus. The values correspond closely to the usual PSE secured by Urban's process of calculation. For purposes of comparison of results from different experiments, however, they are more convenient than the laborious PSE, for they are independent of stimulus-values and modality.

The results in the second column of Table I are taken from a series in which a soft sound was interpolated between the standard and comparison. According to the assimilation-hypothesis, the trace of this interpolated stimulus should interact with the trace of the standard, thus bringing the trace of the latter down to a lowered level. The negative values in the second column show that the hypothesis is borne out. An important difference, however, between the values of the two columns must not be overlooked.

In the first column the values are also negative. Since the days of Fechner negative time-errors have been ascribed to the weakening trace of the standard. Lauenstein has argued, however, that normal negative time-errors are due rather to assimilation,—assimilation downward to an empty background (*Reizlosigkeit*). But if this were true, the size of

the normal time-errors should be larger than those for an interpolated soft stimulus, for assimilation to zero must be greater than assimilation to any value between zero and the standard. This assumption is not supported by the facts. The average value for the normal series is − 0.13, whereas the average for the 'soft' series is − 0.41. Similar results are shown in Table II, where weights instead of sounds were used. For

TABLE II

Values of time-error for two series of judgments on lifted weights. Second series involves interpolation of a light weight.

Os	Normal	Light
B	−0.06	−0.33
M	−0.23	−0.43
C	−0.20	−0.40
Ave.:	−0.16	−0.38

the normal series (no interpolated weight) the average value is − 0.16, whereas for the series with a light interpolated weight the value is − 0.38.

Why does the assimilation-hypothesis break down in this case? Let it be assumed that the course of a given trace, instead of being determined by one factor (assimilation), is governed under certain conditions by the operation of two factors, weakening and assimilation. It must be supposed that weakening always occurs, and that its downward movement will be clearly revealed unless some interpolated trace alters the course of events. If now the interpolated trace produces assimilation in the same direction as that caused by weakening, as it does whenever it is appreciably below the level of the standard, the combined action of these two factors will obviously lower the level of the standard trace more than weakening alone can do. This is exactly the situation in the results of Tables I and II. In the normal series there is no interpolated trace, nor is the trace of an empty background (*die Spur des Zustandes der Reizlosigkeit*) closely enough related to the standard sounds and weights to bring about assimilation of the trace of the standard to that of the background. The change in level of the standard trace must here

be ascribed entirely to weakening. In the interpolated series, however, assimilation and weakening both operate. A soft sound is sufficiently like a louder one, or a light weight like a heavier one, to produce interaction of the respective traces. The level of the standard trace should therefore be lowered more under these conditions—an expectation which the second columns of Tables I and II show is completely fulfilled.

There is a further test of the validity of the hypothesis just proposed. If assimilation alone determines the course of a trace, there is no reason to expect its amount will be more in one direction than in another. That is to say, an interpolated stimulus *below* the standard should not pull the trace of the standard down more than an interpolated stimulus the same distance *above* the standard should pull it up. It is a simple matter to test this assumption experimentally by interpolating, in two different series, stimuli which are equidistant from the standard. Tables III and IV show results from such experiments.

In Table III 62 gr. refers to the value of the weight inter-

TABLE III

Values of time-error for two series of judgments on lifted weights. Numbers at top of columns refer to values of interpolated weights. Standard weight was 100 gr.

Os	62 gr.	162 gr.
A	−0.36	−0.06
L	−0.50	−0.04
H	−0.38	0.06
Ave.:	−0.41	−0.01

polated below the standard weight of 100 gr., and 162 gr. to the value interpolated above. The interpolated weight was lifted just before the standard, and the comparison-stimuli followed the standard after an interval of 10 sec. It will be seen in the first column that the lighter interpolated stimulus produces marked negative time-errors, which means that the level of the standard trace has been noticeably lowered. Whatever assimilation accomplishes in one direction, it should be able to do in the other. The values in the second column should be equally large, but positive. Such

expectation is not fulfilled. The values for 162 are less negative than for 62, but they have not been pulled up nearly so far as the theory of assimilation demands.

The results in Table IV are taken from an experiment

TABLE IV

Data from Guilford and Park (1). Values of time-error for two series of judgments on lifted weights. Numbers at top of columns refer to values of interpolated weights. Standard weight was 200 gr.

Os	100 gr.	200 gr.
G	−0.30	0.05
H	−0.47	−0.15
M	−0.26	−0.07
Ave.:	−0.34	−0.06

reported by Guilford and Park (1). The experiment was designed for another purpose, but the set-up of the stimuli was such as to make it possible to use the results to test the present hypothesis. The standard stimulus was 200 gr., and the interpolated stimuli 100 and 400. The interpolated stimulus was lifted after the standard, and the standard and comparison were separated by 7 sec. For the lighter interpolated stimulus the negative time-errors are again large. But the values for the heavier interpolated stimulus are by no means equally large in the positive direction.

An hypothesis which makes use of assimilation alone is apparently not adequate to the facts. The results in Tables III and IV can easily be accounted for, on the other hand, by assuming that assimilation and weakening both operate. It must be supposed that weakening always operates, and that its direction will always be downward. Whenever assimilation plays a part, its direction will depend on the position of the trace which permits interaction. If the trace is below the standard, the direction will be downward. Since weakening is also downward, assimilation in this case must reinforce the downward movement. When on the other hand, as the result of the interpolation of a stimulus above the standard, assimilation is upward, the two factors work in opposite directions. Assimilation is strong enough to hold the trace

of the standard at its initial level (average time-errors of − 0.01 and − 0.06), but it is not strong enough to pull it up still further, as it should do if there were no other factor working in the opposite direction.

It is thus clear that in order to account for the temporal course of a mnemonic trace, at least two factors must be postulated. One may be called *disintegration*, the other, *interaction*. These words are intended to refer to *formal* physiological properties deduced from the characteristics of certain stimulus-report relationships. They are concepts used as generalizations regarding the way events are ordered in direct observation. What the *material* biochemical properties of neural action are which underlie the observed relationships is as yet completely unknown. The concepts must therefore not be understood as having reference to any properties other than those which may be inferred from the manner in which delayed judgments behave as a function of known stimulus-values.

The facts which lead to the concept of disintegration are similar to those which in other contexts have been used to support the law of disuse. Since certain of these same facts can not be accounted for by interaction, it is maintained that within limits the law of disuse is still valid. The objection that a trace can not possibly disintegrate through disuse, that time in and of itself does nothing to any event in nature, must be regarded largely as a verbal quibble. Science often speaks of change as a function of time, meaning thereby that alterations are produced by processes internal to the event in question rather than by external forces acting upon the event. Let *A* and *B* represent two neural traces. Either one may change as the result of the action of the other upon it. A change may also occur in *A* by way of metabolic processes within its own organization which have nothing to do with *B*, and *vice versa*. There is therefore no reason on these grounds to dismiss the principle of disuse.

SUMMARY

If comparison-stimuli symmetrically distributed about a standard are presented for judgment by the Method of Delayed Comparison, there will be under ordinary circumstances a marked preponderance of 'greater' judgments. This fact has generally been accounted for on the assumption that in the interval between the presentation of the standard and the formulation of the judgment on the comparison, the trace of the standard disintegrates. This assumption, however, is equivocal, for the downward movement of the trace might be produced by assimilation to some lower trace in the comparison-field. By the use of interpolated stimuli it has indeed been shown that the trace of the standard will move in the direction of the interpolated stimulus. This does not mean, however, that the principle of disintegration must be entirely displaced by that of interaction. Experiments have shown (1) that assimilation to zero (no interpolated stimulus or background) is less than assimilation to any value just above zero, and (2) that assimilation to a trace below the standard is greater than to a trace the same distance above the standard. Neither of these facts is consistent with the principle of interaction. They may easily be accounted for, however, if one assumes that disintegration always operates and that assimilation operates only when some trace is in the comparison-field which is closely enough related to the trace of the standard to permit of interaction. Both principles are needed to explain mnemonic changes: disintegration, always; interaction, whenever certain specified conditions are present. These latter conditions are present in abundance in the usual phenomena of memory and forgetting. They nevertheless do not preclude the operation of the law of disuse. They merely tend to obscure its manifestation. In psychophysical technique the law of disuse can be clearly demonstrated and decisively tested.

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TWO BASIC MECHANISMS IN MOTIVATION [1]

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Numerous experiments with both animal and human subjects have demonstrated that food-deprivation has a host of behavioral consequences. It is difficult, however, to demonstrate any simple, and general, relationship between deprivation and behavior. When an attempt is made to relate given behavioral effects to the extent of the deprivation, it is found that every such relationship is more or less unique. Thus gross activity increases for a time with deprivation and then decreases (13). The rate of eating and the amount of food required for satiation decrease with time of deprivation (2). Certain reflexes are periodically facilitated during deprivation (7, 15).

An examination of such varied behavioral effects, each of which seems to follow its own individual law, suggests that there are a number of distinct mechanisms operating in food-deprivation. Two of these mechanisms, the 'proprioceptive,' and the 'sympathico-adrenal,' we discuss in the present paper, maintaining that these two mechanisms are adequate to account for most of the available data concerning the effects of food-deprivation. Of still greater importance, however, is the fact that these mechanisms are not peculiar to the hunger-drive. They are basic motivational mechanisms which serve to account for both likenesses and differences among the various fundamental drives. Moreover, the understanding

[1] It should be noted that Professor R. C. Givler (11, p. 57 ff.) has pointed out the importance of adrenin and of muscular tensions in the genesis of motivation. The present account is substantially in agreement with his more theoretical treatment.

of them serves to bring the facts of deprivation into relationship with the facts of emotion.[2]

The proprioceptive mechanism—The facilitating action of muscular tensions is indicated by a variety of experiments. The amplitude of the knee-jerk is increased by simultaneous clenching of the hand (4). The learning of lists of nonsense syllables and the adding of lists of digits are improved when the subject is exerting pressure on a hand dynamometer (1). The learning of a stylus maze is improved with muscular tension (14). There is other evidence of the same sort to indicate an enhancement of central nervous processes as well as of skeletal reflexes.[3]

This same mechanism apparently operates in hunger. The hunger-contractions of the stomach are accompanied by widespread facilitation. The extent of the knee-jerk is again increased at the time of hunger-contractions (7, 15). Strength of grip and general activity are increased (15). Certain mental performances are improved (15).

Carlson's observation (7, p. 87) that the same facilitation of the knee-jerk may be produced by mechanical stimulation of the stomach throws further light on this mechanism. The extensive effects of hunger-contractions are not due directly to any general nutritive condition of the organism. They are a direct result of afferent impulses set off by the stimulation of receptors in the walls of the stomach.

In so far as we can determine from the available data, the effects of muscular tensions and of hunger-contractions are similar. In each case volleys of afferent impulses result in

[2] The present account is not exhaustive. There may be other mechanisms not yet identified. And obviously the two mechanisms considered are conditioned and re-conditioned, so that in the adult human organism they are brought into action by stimuli far removed from the biologically adequate ones. The problem of learning in relation to these mechanisms and also the implications of these mechanisms for the experimental control of the hunger-drive will be discussed in detail elsewhere (3, 9).

[3] We are aware of the fact that there are also experimental results (10) indicating a disruptive influence of muscular tensions on certain kinds of performance, but feel that this does not invalidate the argument. The proprioceptive mechanism, whether initiated by the contraction of the stomach or of skeletal muscles, has the appearance of increasing the rate or quantity of output; yet it may definitely be a hindrance where highly skilled adjustments and coördinations are required.

the facilitation of skeletal reflexes and of central processes. We may assume that one and the same mechanism is involved in the two cases.

The sympathico-adrenal mechanism—Cannon (**5**, **6**) has furnished much valuable information concerning the action of the sympathico-adrenal mechanism in the 'emergency' emotions. He has also studied the manifestations of hunger. We cannot discover, however, that he has anywhere formally recognized the functioning of the sympathico-adrenal complex in the case of food-deprivation. Possibly this oversight is due to the fact that his own experiments on hunger have been concerned with relatively short periods of deprivation. There is a considerable amount of evidence which indicates that hunger is an 'emergency' condition, that the starving animal is truly an 'emotional' animal. McCarrison (**12**, pp. 156 *ff.*) has recognized the functioning of the sympathetic nervous system in starvation and has presented supporting evidence. He found that the adrenals were characterized by a significant enlargement during starvation. According to McCarrison the condition involves a genuine hyper-adrenalism. This relationship holds not only for complete deprivation, but also for quantitative and qualitative deficiency. That is to say, the sympathico-adrenal mechanism is likewise active in the cases of systematic underfeeding and of deprivation of certain necessary food substances.

These two mechanisms, the proprioceptive and the sympathico-adrenal, account satisfactorily for such varied behavioral effects of hunger as are listed by Elliott (**8**). The fact that there are two mechanisms resolves some of the difficulties encountered in that attempt to list the characteristics of behavior under conditions of deprivation. Rhythmic changes in activity and periodic facilitation of various processes are due to the proprioceptive mechanism which is set off by the recurring hunger-contractions. The presence of a typical emotional condition offers an explanation for a number of other effects. Bousfield and Elliott (**2**) found that with an increase in time of deprivation rats eat more slowly and require less food for satiation. From the facts presented

by Cannon (5) regarding the effect of the emotions on the activities of the alimentary tract these results should have been expected. Warner (16) has shown that the number of crossings in the obstruction-apparatus increases with deprivation for a few days and then decreases. The later decrease has been attributed to the general debility of the animals. A further reason for the decrease may now be suggested. After a few days of starvation an animal's alimentary tract is not in a condition to deal effectively with food because of the increased sympathico-adrenal activity.

The different behavioral results of food-deprivation are now seen to result from the operation of one or both of these mechanisms. The inter-relationships of the two mechanisms and their relative importance after varying degrees of deprivation have still to be determined. Undoubtedly the proprioceptive mechanism is more important under normal circumstances and decreases in effectiveness with long periods of deprivation. Richter (13) has demonstrated that the rhythms of activity associated with hunger-contractions tend to disappear after a time. This disappearance may be due to either the loss of periodicity of the hunger-contractions or to adaptation. It is conceivable that the sympathico-adrenal mechanism plays a part in the suppression of the proprioceptive mechanism.

The sympathico-adrenal mechanism is ordinarily of little importance to the organism having access to an adequate kind and quantity of food at reasonable intervals. It becomes more and more effective with the degree of deprivation. The biological utility of this mechanism is evident. When the restlessness and activity resulting from the operation of the proprioceptive mechanism does not bring the organism into contact with food, the second mechanism enters the picture and serves the end of putting the organism into an emergency condition. All of the bodily reserves are placed at the disposal of the skeletal system even though this state means that the organism cannot handle the food so effectively when and if food is obtained.

So far our discussion has dealt only with food-deprivation

and the hunger-drive. We may well ask what roles the two mechanisms play in the other drives. The ultimate answer must wait upon further experimentation but some suggestions can be made now. It seems certain that the proprioceptive mechanism functions in the drives of thirst, sex, and elimination. Probably also in some phases of maternal behavior, such as nursing. In each case some particular organ is distended or otherwise stimulated and thus gives rise to the necessary volleys of afferent nervous impulses. Since there are usually internally determined rhythmic changes in these organs, the proprioceptive mechanism comes into operation periodically. In the exceptional case of the male sex-drive, where no definite rhythms have been demonstrated, the mechanism works in exactly the same manner. The changes in the organs which set off the mechanism are, however, more dependent upon external stimulation than is the case in the typical internally determined drive. The existence of the sympathico-adrenal mechanism in the other drives remains to be demonstrated. It should be found to be most important in the thirst-drive, and possibly of relatively less importance in some kinds of maternal behavior. It is probable that the sympathico-adrenal mechanism functions only rarely, if at all, in the sex and elimination drives.

From a review of the experimental evidence upon the behavioral effects of deprivation two facts stand out: first, that the various drives are rather similar in their effects and, secondly, that in certain respects the drives are like the emotions. The reasons for these two facts are now evident. The different drives have similar effects to the extent that the proprioceptive mechanism operates. A particular drive acts like an emotion in so far as the sympathico-adrenal mechanism is involved.

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GRAY'S OBJECTIVE THEORY OF EMOTION

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In the issue of the PSYCHOLOGICAL REVIEW for January, 1935, Gray has criticised the suggestion that emotional experience has a thalamic origin, and in place of it has offered what he calls 'an objective theory.'[1] I desire to examine both Gray's criticisms and his theory.

Gray states that "Cannon has no evidence that the thalamus adds feeling tone." One need not quibble over the expression 'feeling tone.' Its significance is not greatly different from that of Gray's '*feeling*' (italics his) which he attributes to 'internal stimulation.' As to the charge of lack of evidence, the interested reader will find in a publication by Cannon (1929, p. 366) a section where, in support of the thalamic source of emotional feeling, the testimony of human beings is cited. When, because of unilateral damage to the cortico-thalamic tract of the cerebrum, there is unilateral release of the thalamic region from cortical inhibition, agreeable affective stimuli are felt much more keenly on the damaged side and are attended by report of intense pleasure and signs of enjoyment or exclamations of delight. Furthermore, singing and the playing of music arouse such increased emotional feelings on that side that they may be intolerable. Similar affective stimuli influencing the normal side, where the relations of cortex to thalamus are unimpaired, have only ordinary effects. Since the sole difference notable between the two sides of the body *in the same individual* is the unilateral cerebral lesion, the conclusion is drawn (*loc. cit.*, p. 368) that the evidence indicates that the processes aroused in the liberated thalamic centers become the source of vivid affective experience. The difficulties which this evidence presents for

[1] Absence from the United States for seven months made me unaware of this article until my recent return.

Gray's theory will be considered later. For the present it is sufficient to point out that Gray's statement that there is 'no evidence' for the thalamic contribution to the complex of an emotional experience is quite unwarranted.

Associated with the indications that processes in the thalamic region are the source of emotional feeling are the observed facts that emotional expressions have their neural centers in that region (Bard, 1928) and that unilateral injuries in the region result in unilateral abolition of the typical emotional *facies*. Experimental destruction of the diencephalon (which includes the thalamic centers) results instantly in a disappearance of the emotional display (sham rage, for example), and its replacement by decerebrate rigidity. These observations relating the thalamic region to emotional behavior and experience Gray utterly disregards. Instead he places the origins of feeling in the periphery.

Gray states, "All the endocrines function during all emotions but in different proportions." "Each emotion has its characteristic endocrine function pattern and consequently its blood chemistry formula." It is, perhaps, rather absurdly supererogatory to note that these statements exhibit the outer fringe of unsubstantial phantasies. There is not the slightest shred of evidence that "all the endocrines function during all emotions." The claim that "each emotion has its characteristic endocrine function pattern" is the wildest type of theorizing, without any basis whatever.

Proceeding from the fantastic notion that all the endocrine glands operate in different proportions in the different emotions, Gray next postulates that there are proprioceptors located in the vascular system which would be sensitive to these various chemical formulæ in the blood. "If each emotion," he remarks, "is characterized by distinctive blood chemical composition, certainly the proprioceptors would be sensitive to the difference."

To support his idea of vascular proprioceptors Gray did not go to the sources and learn of the conditions under which nerve endings in blood vessels are stimulated. He wrote to various physiologists asking the question, "Do changes in the

chemical composition of the blood initiate neural impulses in the vascular proprioceptors?" 'The vascular proprioceptors' are in the heart and the aortic arch and in the carotid sinuses. They are sensitive to alterations of blood pressure and sensitive also to 'changes in the chemical composition of the blood.' But the blood changes which affect these areas are variations in the carbon dioxide and oxygen content (see Heymans, Bouckaert and Regniers, 1933). Naturally enough the physiologists who were consulted wrote that chemical changes in the blood stimulate vascular proprioceptors. That testimony, however, gave Gray not the least justification for proposing the fanciful idea that various combinations of internal secretions can influence the sensory nerve endings of blood vessels in distinctive ways. Stiles, one of the physiologists who was consulted, has declared that such an application of his testimony never occurred to him. And I venture to assert that the other physiologists, likewise, would repudiate Gray's use of their statements.

Obviously some of the physiologists reported not only on the action of chemical agents on the proprioceptors, but also on the nerve endings of *effectors*. Whichever it was, it was grist to Gray's mill. Thus he quotes Greene's remarks as follows: "We physiologists look upon the hormone of the adrenal medulla as a specific substance reacting with the myoneural terminations of not only the vascular autonomics arising from the thoracic cord, but with end-organs of various other so-called sympathetic pathways." Carlson likewise mentions that "a great many substances in addition to adrenalin act on peripheral nerve endings" (Gray, 1935, pp. 114, 115). Now 'myoneural' implies effects on muscle, not on receptors, *i.e.*, the action of adrenalin is in the *efferent* path. What is Gray trying to prove? He asks physiologists about chemical stimulation of *proprioceptors*. And when they testify to chemical action on *effectors* he quotes that testimony as if it were pertinent! It is well to remember that more than 100 years ago the difference between afferent and efferent nerves was made clear. If the reader is careful, Gray's confusion will not serve to support his 'objective theory.'

So eager is Gray to bolster his contention that hormones rouse emotion by stimulating proprioceptors that he quotes the observations of Landis and Hunt (1932) on the subjective effects of injection of adrenalin. What these observers actually noted was that in most cases there was a 'cold emotion'—the subject feeling 'as if' afraid, 'as if' in great joy. In a *few cases only* was there a complete emotional experience. In the similar studies by Cantril and Hunt (1932) and by Marañon (1920) such completion occurred again only in a few persons. Now Gray, neglecting the large number of indifferent cases and paying attention only to the few, draws the conclusion that "without question changes in blood chemistry do initiate neural impulses which are interpreted by the individual to be emotional feelings." Does the change in 'blood chemical composition,' due to the presence of adrenalin, stimulate proprioceptors? What proprioceptors, and where? Adrenalin does, indeed, act on structures innervated by the sympathetic system, as do sympathetic nerve impulses, and thus it causes high blood pressure, and a pounding heart beat, and an altered distribution of blood in the body, and an attendant perceptible throbbing, and an increase of muscular tension. These changes one may feel, but Gray alone has any idea that adrenalin acts directly on vascular receptors—and he has shown us no evidence for it.

A virtue attributed by Gray to his theory is that it "accounts for the fact that emotion is felt 'all over.'" The angry person feels that way, Gray claims, "because he is feeling his own blood"; the blood is everywhere, proprioceptors are everywhere (working on the chemical formulæ) and hence the all-overishness of the experience. But what shall be said of persons who, because of damaged cortical control of the thalamic region on one side, have unilateral emotional feelings (see p. 1)? The endocrine formulæ cannot be restricted to one side of the body, nor the blood flow. And what shall be said of such cases of fractured cervical spinal cord as Dana described? According to Dana his patient, an intelligent woman (whose body below the neck was connected with the brain only by the vagus nerves) declared

that there was no resultant alteration of her emotional feelings (see Dana, 1921). If a supposed all-overish quality, before her accident, was due to feeling her blood formulæ in all parts of her body, why no change when the formulæ could affect only a small remnant of her anatomy? Theorizing is easy if one discards disturbing facts.

Gray characterizes the suggestion that emotional feeling has its source in neural processes in the thalamic region as a 'bit of mentalistic speculation.' It would appear that there is a derogatory implication in the label 'mentalistic.' Perhaps, however, Gray would be willing to admit that the relation between processes occurring in nerve cells and in neural patterns, on the one hand, and the subjective experience of feeling, on the other, is not quite well known, is still somewhat mysterious. The thalamic theory of emotion, the evidence for which has been reviewed above, postulates that the nerve impulses aroused in neurone patterns in the thalamic region are the occasion for the peculiar *quale* of an emotion, the emotional feeling. Gray states that "the feeling of fear is the internal stimulation aroused by the individual's own blood chemistry . . . a proprioceptor stimulation caused by the chemical composition of the individual's own blood." As we have seen, that idea has not the slightest factual support—indeed, as little claim to reality as the shadow of a phantom. And yet, if vascular proprioceptors were stimulated, afferent impulses would pass to the brain and would be the occasion for the arousal of emotional feeling. Nerve impulses would be involved, therefore, in either case. Why nerve impulses roused peculiarly by stimuli affecting the thalamus should suggest branding their proponent as "mentalistic" and "vitalistic," while nerve impulses assumed to come from somewhere in the periphery should be a warrant of that objectivity which we investigators all crave—that needs further exploration.

Thus far the assumption has been made that for the experience of emotional feeling Gray's theory requires the existence and functioning of the brain. Gray does not mention, however, the necessity of cerebral function either for the inter-

pretation of the nature of the emotion-provoking stimulus or for a response which is properly adapted to the circumstances. In his final paragraph he declares that emotion is intense bodily behavior initiated by stimulation which evokes 'immediate' action of the endocrines, with 'resultant change in blood chemistry' and in the viscera. Thus the internal receptors are stimulated "and the organism feels emotional. He feels himself behaving in that way called emotional. Different emotional feelings are distinguished because blood chemistry and other physiological changes are so characteristically different for the various emotions that the proprioceptors are stimulated differently. This is no psychic state but physical behavior in response to internal stimulation." Although Gray seems not to need the brain in order to explain emotion objectively, actual physiological experimentation proves that it has some importance for the organism when 'behaving in that way called emotional.' Bard's decorticate cats, if stimulated by being pinched, did not react by biting the experimenter, but instead commonly bit their own bodies (Bard, 1934). And Goltz's decorticate dog had no fear of the brandished stick (Goltz, 1892). Neither in anger, nor in conditions naturally arousing fear was the behavior appropriate. It would appear, therefore, that the neural pathways in the cerebrum do play a rôle in the natural course of an emotional response. And, as has been shown above (p. 1), they have importance also for the experience of emotional feeling.

Gray's charge that Cannon's thalamic theory of emotion is supported by 'no evidence' has been proved to be unjustified. His own theory of 'an objective theory of emotion' has been shown to rest on baseless surmises. His unqualified and quasi-authoritative assertions should not be accepted as statements of facts.

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